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Editorial Notes

THESE Notes faded out of existence early in the War, because under the shadow of that great storm it seemed frivolous to continue the discussion of abstract ideas.

In England archaeological work did not come to a standstill; indeed, thanks to the initiative of a government department and the dauntless energy of some of its servants, the excavation of 'military' sites was continued, winter and summer, for six years without remission, with a rich harvest of new knowledge. But what one might call the social side of archaeology, with its meetings and opportunities for discussions and the exchange of ideas, was necessarily in abeyance. These Notes aimed at reflecting the ideas and problems of the archaeological world and they would have been empty of content during those terrible years. We were all preoccupied by other things. The Editors considered that the prime task of ANTIQUITY was to keep going till better times should come. This we have been able to do, thanks to the unswerving support of our readers, which alone made this possible. More than once we have been told that it was our duty to keep ANTIQUITY going whether we liked it or not! We were assured that it had achieved a unique position in the world, and that its discontinuance would be universally regretted. These assurances were so strongly expressed that, quite apart from any opinion of our own, we knew they must be true. The fact is that ANTIQUITY has become an institution, representative of the best and liveliest elements in the world of Archaeology; and it has a momentum of its own. The Editors are the servants of this institution, just as in a democratic state the ministers are the servants of the public that elects them. That is the natural and proper result of following a correct policy right from the start. The Editors may justly take credit for formulating the policy, but they willingly share the credit of its success with all those who, by their unwavering support, have ensured its success. Without that support the policy could not have succeeded. In short, ANTIQUITY survives because it was founded upon correct principles.



We shall try and return gradually to our original format, but paper restrictions still make this difficult. There are other difficulties—increased cost of materials, lack of draughtsmen (especially for maps), difficulties of communication. Moreover, many of our contributors are still occupied with the aftermath of war, and some of them, alas! have died. Owing to the state of Europe we are thrown more on our own resources. It is not easy to discover what is going on in the world outside, and one fears that for a long

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time to come the primary tasks of reconstruction will necessarily absorb the whole energies of peoples. But when once the fundamental requisites of food, clothing and shelter have been secured, there is much reconstruction of cultural life to be undertaken. Much has been destroyed, but not quite all; and even if every relic of the past had vanished and we had to begin again, we should do so, because our task of reconstructing human history remains. Nothing can destroy that so long as the technique of archaeology is practicable. It is difficult to imagine any convulsion so great and overwhelming as to obliterate every field everywhere in which sites like Woodbury may exist, together with the skill to exploit them; and modern warfare at any rate has not shown any signs of eliminating air-photography.

Not long ago we published an article (ANTIQUITY XVIII, 147-50) advocating an institution of higher education in West Africa. Such an institution already exists at Khartoum. If it is true, as so often stated, that we Europeans are installed in African countries for the benefit of the inhabitants rather than to exploit them, then it is by just such institutions of higher education that we can best fulfil our purpose—and not only in the sphere of the past. The policy of the U.S.S.R. in the Asiatic republics is an example of what can be achieved, when the predatory methods of big business have been eliminated. The British have been governing India for a long time, but so far there has been no serious or organized attempt to instruct Indians in modern archaeological technique. Not only is this true, but even the British do not appear to have applied such methods as air-photography and post-hole technique to the few sites explored or the many that might have been discovered by one or other of these devices. We are therefore, perhaps, on rather delicate ground when we criticize Indian archaeology, as it is criticized in a review published in this number. Nevertheless we have decided to publish it in exactly the form in which we received it because we feel that, whoever may be responsible for the present state of Indian archaeology, the facts as stated by our reviewer are correct. We receive for review quite a number of publications written by Indian archaeologists; the majority of them consist, almost entirely, of worthless speculation or unintelligible discussions of literary texts. Such are not, properly speaking, archaeology at all. Archaeology is concerned with the soil, in which Indian history is buried. It will remain buried there until it is unearthed by modern methods. It is the task of Indians to extract it, and it should be ours to give them the tools and teach them how to use them. If they have much to learn, we have neglected opportunities to atone for. Let us each admit our shortcomings and start afresh on new lines. There is a wealth of opportunity.

The recent appointment of Dr Wheeler to the post of Director-General of Archaeology in India inaugurates a new era. Dr Wheeler symbolizes the British School of Archaeology in Britain, which is the best in the world. His superb leadership, enthusiasm and mastery of technique have already begun to revolutionize a department which, long dead, was rapidly decomposing. His finely unconventional official Memoranda are clarion-calls that should rally to his side everyone, in Whitehall as well as in India, who is genuinely interested in Indian history and culture. They are inspired by that practical horse-sense which characterized the British before they were strangled by their own Treasury. One can imagine the feelings of horror and amazement with which his

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Memoranda will be read by pen-pushing civil servants everywhere. 'Learn by watching coolies at work . . . Learn by trial and error how to mix your mortar . . . You are no longer a schoolboy waiting to be taught'. 'Science is a *whole-time* pre-occupation. It has nothing to do with office hours. There is no such thing as "science from 10.30 a.m. to 5.30 p.m." Those are the hours between which the administering scientist has *least* time for his science. His real work begins when his routine work ceases. And archaeology is a branch of science'. The example of Soviet Russia is held up for imitation; just as that country imported capitalist technicians to build up a socialist industry, so should India import archaeological technicians for a limited period to train Indian workers in their methods. The gist of it all is 'Deeds not words. Away with all this spate of verbiage and literary drivel; let us get down to the job of unearthing the history of India and conserving its national monuments properly; and in particular let us begin by filling up the gap of 2000 years between the end of the Indus culture and the coming of Alexander'.

We wish Dr Wheeler the best of luck in his attempt to hustle the East (which includes of course Anglo-Indians). If, as we expect, his influence on Indian archaeology proves ultimately to be as deep and as permanent as it will be on British archaeology, then he will have gone far to atone for the neglect of decades; and he will also show that, if the British are the most uncultured people of the world, they do also occasionally produce the antidote.

As we go to press we have received the first number of a splendid new publication—*Ancient India*: Bulletin of the Archaeological Survey of India. A full notice of this will be published in the June number; meanwhile we heartily congratulate the Survey and wish *Ancient India* a long and prosperous career. It is priced two rupees, or three shillings and is to be obtained from the Manager of Publications, Delhi, India. The contents are not too technical to appeal to any reader of ANTIQUITY.

A Hill-fort in Switzerland

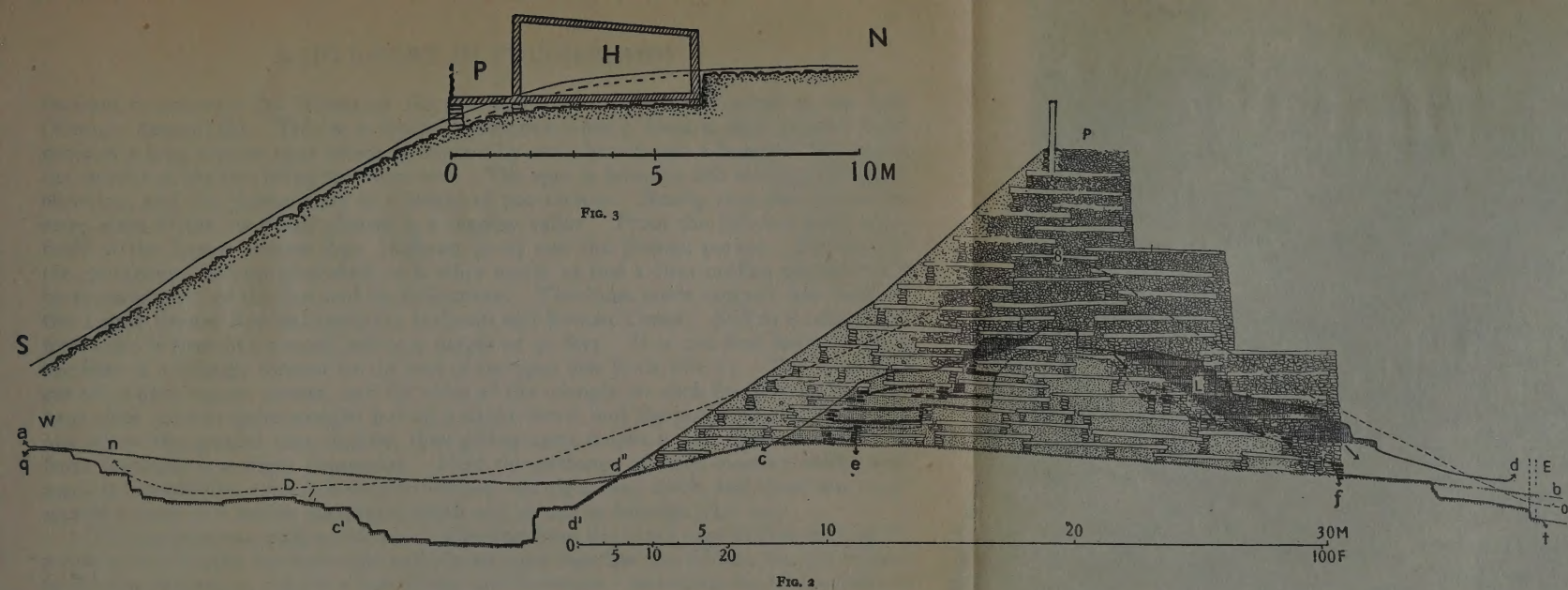
by GERHARD BERSU

THE great cultural unit of the Hallstatt (A-B) Period in southern Germany and its neighbourhood is now considered to be the cradle of the Celts. The latest discussion of the problem is that by P. Bosch-Gimpera (1). A special variety of this culture seems to be represented in the country lying between the Alps and the Jura, extending from the lake of Constance to the lakes of Savoy. It is found in the lake-dwellings which came to an abrupt end somewhere about 700-650 B.C. The top layer in many of these is marked by evidence of burning, indicating a catastrophe that must have been wide-spread and general. This type of habitation, once so prevalent, was never revived; there are no signs of it in the following period (Hallstatt C-D). From the Late Bronze Age onwards there was a continuous development in the lake-dwellings, whose last stage is called in Switzerland the Latest Bronze Age (contemporary with the Rhenish Hallstatt B).

A century of collecting, dredging and digging produced large quantities of impressive finds, derived from the easily accessible shores of the great and small lakes. These remains, together with the spectacular remnants of wooden habitations, and the romance of popular literature, brought into existence a special and peculiar conception of this civilization. More or less explicitly the idea took shape that these people were so fond of living on the shores of lakes, that they developed, *qua* lake-dwellers (Pfahlbauern) peculiar characteristics of their own. Protected both by water and by the mountain-clad forests around, they were supposed to have flourished as peace-loving people, skilful in crafts and living by agriculture and fishing until overwhelmed by a tragic end. Some few habitation-sites, it is true, situated away from the lakes—even in the 'protecting' mountains and forests—had long been known. But, as always, such sites yielded only poor finds. The then still primitive art of excavation revealed no substantial remains of dwellings. As a result, the existence of these inland habitation-sites was conveniently forgotten when the aforesaid conception of specialized lake-dwellers was formed.

In recent years, however, the growing interest in Swiss prehistory has been responsible for the discovery of an ever-increasing number of these habitations on terra firma, contemporary with the lake-dwellings. Finds gradually accumulated, derived either from accidental discoveries or from minor excavations in the flat, open country, some of them from naturally protected sites precariously situated on high cliffs. Some also came from the promontory forts common in the mountains. It soon became necessary to revise the traditional conception of the Latest Bronze Age culture in the Alps-Jura zone; it was evident that those features which were typical of this culture in the Rhineland and in eastern France occurred also in this proto-Celtic homeland region. The time was ripe for a large-scale excavation, undertaken for the purpose of showing what a habitation-site in the mountains was like. The opportunity came with the

¹ *Two Celtic waves in Spain*, Sir John Rhys Memorial Lecture; British Academy, London, 1939. See also Christopher Hawkes: 'Problems of the Bronze Age and the beginning of the Early Iron Age' in Conference on the problems and prospects of European Archaeology, 1944. (Occasional Paper no. 6, Institute of Archaeology, Univ. of London, p. 53).



WITNAUER HORN



decision to excavate the Wittnauer Horn, a large hill-fort in the Fricktal in the Jura (Kanton Aargau) (2). This is a typical promontory-fort; from a high plateau there projects a long narrow spur which is crossed by two short banks a hundred feet apart, the smaller of the two being the outer one. The spur is between 660 and 640 metres in elevation, and the plateau rises to a height of 700 metres. Nearly 1000 feet below the steep sides of the spur and plateau is a marshy valley. From the hill-fort have come finds of the Latest Bronze Age, Hallstatt (C-D) and the Roman period. Fortunately the occupations had not disturbed each other much, so that a clear outline picture could be reconstructed of the fort and its habitations. The huge inner rampart was made in the Latest Bronze Age and reused in Hallstatt and Roman Times. Still in its decay 120 feet wide, it rises like a small hill to a height of 30 feet. It is 250 feet long, forming the base of a triangle formed by the rest of the spur (see PLAN, FIG. 1). The area thus cut off is 9000 square metres, and the sides of the triangle are each 800 feet long. The bank does not run quite straight but on a slight curve, and the point selected for it was one where the ground rose slightly, thus giving extra height to the rampart itself. In front (outside) is a slight depression. Here the presence of hard massive oolitic rock made it impossible—even if it were so wished—to dig a deep ditch, and there was made merely a ditch of 6 feet in maximum depth and 50 feet in breadth (FIG. 2D).

The rampart was made as follows:—Starting from the inner edge of this ditch (d'-f) a row of tree-trunks 100 feet wide and placed close together was laid on the old surface of the ground (FIG. 2, a-b) in a bed of clay and limestone; the trunks lay horizontally in an E.-W. axis. At a vertical distance of two to three feet were placed more trunks also reinforced by small stone packings, the intervening space between the trunks and the packing being filled by stones and clay rammed in hard together. In the upper part of the middle and at the back this method of construction was changed, and loose limestone only was used as a bed for the trunks and for filling. The clay and stone bedding was preserved only in a zone towards the outer edge. This structure caught fire in some great catastrophe. The heat of the burning trunks at the back and in the upper part turned their bedding of loose limestone to lime, as in a kiln. As a result of this the rampart became a ruin 150 feet wide with its top 24 feet above the old surface-line, the whole being covered by a layer of solid lime (L) mixed with charcoal about three to four feet in thickness, and also by detached portions of material derived from the collapse of the upper part of the original rampart (n-o, FIG. 2). Under all this the core and lower part of the original rampart are preserved. From the large quantity and the arrangement of the material in a secondary position a reconstruction of the original scheme is possible. The highest point of the rampart was certainly once 40 feet above the inner margin of the ditch, at a point 70 feet away from it; the bottom of the outer portion is still preserved (FIG. 1, d'-d"). By prolonging the angle of slope we get the outline of the outer face of the rampart; it is a steep angle. The inner face was not built up straight. Three masses of steeply inclined collapsed material alternate with two others less steeply inclined, at the back of the rampart. These flatter portions represent two horizontal spaces set between three steep fronts. Of the top of the rampart we only know that it was flat and about 10 feet wide. Now it is obvious that the object of constructing this huge bank was not merely concealment from an enemy. So we conclude that there was a walk on the top of the rampart, to which access was provided by the horizontal stretches or ramps at the back. The walk was, we can be sure, protected by

² See the forthcoming vol. IV of the series, *Monographien zur Urgeschichte der Schweiz*: Das Wittnauer Horn in Kanton Aargau. Gerhard Bersu. 118 pages, 42 plates, 4 maps. Basel, 1945.

a parapet, as shown in FIG. 2 (P). We could not discover how the rampart was finished off at the north and south ends where it rests on the natural slope of the hill (which has the same angle of slope as the rampart itself). It is certain, however, that the entrance can only have been at the north end.

If we take into account only the 200 feet length of the rampart, we find that it had the enormous cubic content of 24,000 cubic metres, one third of which was timber; for the whole construction may safely be added another 6,000 cubic metres.

The defensive system of the rampart is very simple. By building a bank across the neck of the spur having the same angle of inclination as the natural slope of the hillside, the defenders created the same conditions as existed round the sides of the spur. The rampart is thus simply an imitation of the natural defence, achieved at the cost of enormous labour; no kind of device suggesting any imagination or adaptation to special conditions can be found. The scheme originated simply from the observation that natural slopes provide defence; and it is obvious that we must be near the origin of this simple type of fortification. There are indications that such ramparts were a common feature of the proto-Celtic Hallstatt A-B culture and that they originated in it. This defensive principle is different from that of walls with vertical faces so common in the Near East a thousand years earlier. It is an adaptation intended for the special method of warfare adopted in the Hallstatt A-B culture, where dead ground in the angle at the foot of a steep vertical rampart-wall had to be eliminated. The slingstones found in the ditch show that neither swords, daggers nor bows and arrows were the usual weapons of offence to be encountered. The weapon which motivated the construction of such ramparts was certainly the slingstone (3). Dr Mortimer Wheeler, in his Report on Maiden Castle (4), has dealt with the influence of the slingstone on the shape of Iron Age forts. Here we have, half a millennium earlier, an example of a different kind, showing how a lofty defensive stance (the rampart-wall) is artificially constructed in order to outrange the attack; and how this defensive line is made very broad so as to withdraw the inhabitants and place them out of range of sling-fire. It must have required bitter experiences to bring about the building of such huge ramparts which rival in size the defensive works of later Celtic times. That which, on both sides of the Channel, looks like a mania for huge defensive works in the last centuries B.C. is now seen to be deeply rooted in the heritage of the past.

Our views are confirmed by the masses of slingstones which we found spread out on the house-sites or piled in ammunition-dumps round the margin of the interior (FIG. 1). There is a gap in the otherwise continuous row of houses exactly where in the southeast the cliff is almost vertical, and therefore unclimbable; and this again underlines the importance of natural defences in the design. For security and defence purposes the builders ignored the disadvantages of having the houses perched on the shoulders of the spur, where they must have been exposed to all the inclemencies of the weather. The disposition of the houses shows that their lay-out was part of a well-conceived intelligent plan, which if it occurred in a later period would certainly have been interpreted as a purely military establishment. Nothing is left to chance. Behind all these arrangements we can discern the same strong organization as that which caused individuals to cooperate in the great labour of building the rampart.

³ The Wittnauer Horn slingstones consist of carefully selected pebbles of diluvial origin and about the size of a child's fist. They must have been collected in the river valley, as on the plateau they do not occur in sufficient numbers.

⁴ 12th Research Report, Soc. of Antiquaries of London, 1943, pp. 48 ff.

A HILL-FORT IN SWITZERLAND

It is not difficult to guess how exactly the houses fitted into the scheme of defence. They were built upon horizontal terraces levelled out of the rock on the shoulder of the hill (5). On these was laid a substructure of horizontal beams. The ends of these, which, owing to the steep slope, stuck out into the air (FIG. 3), were supported by packing-stones. Since the houses fell into ruin much soil has been washed down the hillside, but an average breadth of 15 feet for each house is certain. As it was intended to build these houses in rows, one touching the other, and as the shoulder of the plateau slopes to the east, rock had also to be cut in a w.-e. direction to obtain a series of level terraces for the substructure (6). The length of the house is got from the steps between the different levels; it varies between 10 and 30 feet, averaging 20. We also found that each house had one hearth, thus showing that it contained one family. The houses were continuously inhabited and many alterations were made. Terraces for new houses were cut back against the original inner wall; new houses were built at a higher level on the shoulder; and the walls separating houses were also moved. The details of all these reconstructions were not everywhere unravelled; but enough was done by excavation to show that this coordination of lay-out and defence was maintained throughout the whole existence of the settlement. At no time were houses built in the gap in the southeast. The earliest stage of the lay-out is given on the plan (FIG. 1, oblique hatching). Note the significant fact that, in the north and south, just where the shoulder of the plateau changes direction slightly, houses project from the row like bastions. It is from just these points that the defenders, by flanking fire east and west, could help considerably in the defence of the slopes. Obviously they could not take up positions *in* the houses. Large openings in the house-walls are out of the question. We must therefore assume the existence of some kind of continuous parapet-walk outside the houses. The labour of constructing such could have been combined (as a single undertaking) with that of building the houses, and thus their position on the slope could have been fixed at the same time. Our reconstruction (FIG. 3: H=House, P=Parapet) gives a probable explanation of how these rows of houses were used in defence. No structural difficulties would have been involved by adding a second parapet-walk at roof level, where a more intensive defence seems required.

There were some 30 such house-units in the southern and 35 in the northern row. At the junction of the two in the east, one house was placed astride the narrow ridge at the end of the inhabited area. The floor was sunk a foot into the ground. Four similar houses, also several times rebuilt, stood in a more sheltered position on the inner part of the plateau. There is nothing in the formation of the ground to account for their being placed here, and it would appear therefore to have been part of the original design to leave two open spaces inside the area. That they were dwelling-houses is shown by the hearths in them.

Altogether there are some 70 houses; so that, assuming 6 people for each house, there must have been about 400 people permanently resident in the fort. There were no such things as reservoirs or ponds for storing water within it, and the nearest springs with a copious supply are 300 feet below the plateau. Normally, therefore, the community had to get its own water and that required by the cattle from these. If the rainwater from the roofs of the houses were collected (as is still done today on the high

⁵ Precisely similar house-platforms are to be seen on the shoulder of Hambledon Hill, Dorset, a hill-fort that may be contemporary with the Wittnauer Horn. See air-photo in *Wessex from the Air*, O.G.S.C.

⁶ The principle of building houses on platforms is of course one adopted in the lake-dwellings.

Jura plateau), enough water could be obtained to tide over an emergency. For this reason, in the reconstruction the roofs have been made to incline inwards.

Not one of the houses shows any signs of having been occupied by a person of high social standing. Everywhere the thick habitation-layers yielded the same kind of pottery, which differed in no way from that found in the upper layers of the lake-dwellings. It was a community of people of equal social standing that lived in the fort, just as in the lake-dwellings. The situation of the settlement on the high Jura plateau, and the lack of special accommodation for storing grain (so commonly found in the open Hallstatt A-B settlements of the Rhineland loess-country) both indicate that cattle-raising was the chief occupation. The very numerous animal-bones from the habitation-layer show that ox, pig and sheep were more common than horse and goat. There are two breeds of ox—one small and the other large, the latter being used for drawing carts. The animals hunted in the forests were red and roe deer, bear, wild pig, badger, beaver and hare. All the breeds of domestic animals (including dogs) are identical with those from the lake-dwellings. There can be no doubt that the fort was built and inhabited by people who had the same culture as those of the Latest Bronze Age lake-dwellings. Moreover, from the fact that pottery identical with that from the upper layers of the lake-dwellings (e.g. Alpenquai Zuerich) is here covered by the debris of the rampart after it was burnt, we know that the same catastrophe which annihilated the lake-dwellings occurred here also.

We know that in the Alps-Jura zone, fortified settlements occur side by side with unfortified ones. In the light of our discoveries at the Wittnauer Horn, it may be concluded that the Latest Bronze Age lake-dwellings—and, in fact, all prehistoric pile-dwellings—are merely a special adaptation to local conditions. In this case it was the presence of a number of lakes conveniently situated. The picture would be even more complete if the ground-plan of one of the *unfortified* settlements in the Alps-Jura zone could be obtained. The proto-Celtic Hallstatt B culture has a uniform aspect. Forts, or defended positions, like lake-dwellings, are self-contained communities. Up to now archaeology has provided only slight indications of tribal organization, and of well-defined areas occupied by different tribes within the great proto-Celtic region. Some of the Latest Bronze Age settlements in the Jura are so obviously placed to form obstacles barring or controlling communications, or protecting access to special areas, that they must certainly be regarded not as independent communities but as part of a larger territorial organization under a single government. This implies tribal organization with distinct communities forming part of a single tribe, and, as at the Wittnauer Horn and in the lake-dwellings, communities consisting of people of equal social standing. On the other hand, the strong organization revealed by the Wittnauer Horn suggests that both the open and fortified settlements of a single tribal unit were ruled by a higher authority—a 'king'—as we know was the case later. This political system may therefore date from the beginnings of Celtic organization.

Pleistocene Chronology in the Far East

by W. E. LE GROS CLARK

TO the anthropologist, the dating of the fossil remains of prehistoric man in the Far East has always seemed a vague and haphazard business. So far as Europe is concerned, we have accepted the correlations between cultural sequences and stratigraphical data which have been worked out after many years of intensive work by geologists and archaeologists. Indeed, so well has the evidence been clarified for us that we even feel we can to some extent assess the validity of arguments put forward for the chronological position of this or that piece of human fossil. But the Far East is very different. The cultural sequences characteristic of Europe are not to be found there, the animal and plant remains found at different stratigraphical levels have a strange oriental appearance and differ specifically too much from those in the European Pleistocene to permit of direct faunistic correlations, and in the tropical regions such as Java the fluctuations of climate related to the glacial periods were not striking enough to provide a reasonable time scale by reference to any deposits so distinctive as boulder clay. Thus the anthropologist in the past has simply noted the personal opinion of local geologists on the antiquity of fossil man or palaeolithic cultures in the Far East, without really being clear as to the evidence on which the opinion was based (and suspecting, sometimes, that the geologist was not always quite clear himself).

Now, in an outstanding publication from the Peabody Museum of Harvard University, (1) Dr H. L. Movius has collected and marshalled all the evidence bearing on Pleistocene stratigraphy in Southern and Eastern Asia. As a result, light begins to appear on this very difficult subject, and the establishment of regional correlations at last makes it possible to base approximate estimates of the antiquity of important fossils such as *Pithecanthropus* on sound reasoning rather than loose conjecture.

The first important question discussed by Dr Movius concerns the criteria used for defining the beginning of the Pleistocene, an absolutely essential preliminary to a study of the sub-divisions of the Pleistocene. In this discussion, he follows the lead given over thirty years ago by Haug, and taken up in this country more recently by Hopwood, by stressing the significance of the faunal sequence. According to this point of view, the opening of the Pleistocene in Europe was heralded by the rather abrupt appearance of new types of mammal, especially *Elephas*, *Equus* and *Bos* (the so-called Villafranchian fauna). According to Movius, this criterion holds good also for Asia (apart from certain local differences), for at the very base of undoubted Pleistocene deposits in Northwest India and North China there appear for the first time elephants, horses and cattle, and also (incidentally) the earliest camels. By reference to the Himalayan region, this conclusion has been strengthened by the observation that these mammals are to be found in beds which were evidently laid down during the first Himalayan glaciation, and it is hardly possible to doubt that this was contemporaneous with the

¹ *Early Man and Pleistocene Stratigraphy in Southern and Eastern Asia*, by Hallam L. Movius. Papers of the Peabody Museum of American Archaeology and Ethnology, Harvard University, vol. XIX, no. 3, 1944.

Gunz glaciation of Europe during which (it is contended by some) the deposits containing the original Villafranchian fauna were formed (2).

Another criterion which is relied on by Movius for establishing the boundary line between Pliocene and Pleistocene is that, at this particular time in Asia, powerful crustal movements and mountain-making processes occurred on a large scale, leading to conspicuous unconformities easily recognisable by the geologist. Finally, it seems that the beginning of the Pleistocene in many parts of Asia was associated with a definite deterioration of climate, even in those regions far removed from the scene of actual glaciation. Indeed, there appears some reason for supposing that pluvial and interpluvial periods as determined by geological evidence in, say, Burma or China, were concomitant with the glacial and interglacial periods which have been demonstrated to have occurred in mountainous areas such as the Himalayas. Movius even suggests that in Northwest India, Upper Burma and North China the climatic records are sufficiently explicit to permit the establishment of a seven-fold system during the Pleistocene (equivalent, that is, to four glacial and three interglacial periods). In Java, on the other hand, partly due to its equatorial position and partly to its comparatively recent emergence from the sea as a single large island (at the end of the Pliocene and in early Pleistocene), the geological record of climate change is hardly legible. Chronological correlations between Java and the mainland can therefore only be made on a faunistic basis.

If climatic fluctuations of a rhythmic character can be definitely inferred from the study of Pleistocene deposits in the Far East, and if these fluctuations can be definitely equated with those now known to have occurred over the same period of time in Europe, the problem of chronological correlations in the two continents would appear to be well on the way to solution. But perhaps a word of caution is needed here to guard against undue optimism. It was not so long ago that controversy raged with considerable acrimony between the monoglacialisist and polyglacialisist exponents of Pleistocene geology in Europe, and, for all the present writer knows, the controversy may be by no means dead yet. Considering the paucity of our knowledge of Asiatic stratigraphy (in comparison with that of Europe), it is probably too much to suppose that geologists are likely to be in agreement for some time on the interpretation of the evidence for climatic rhythms in the Far East. And, until the geologists have reached a decision (at least by a majority vote), the anthropologist will be wise to reserve his opinion.

In the year 1932, a paper appeared in the *Journal of the Burma Research Society* by T. O. Morris, entitled 'A Palaeolith from Upper Burma'. This actually marked the discovery of what has now (as the result of further field studies in 1937-8 by the American South-east Asiatic Expedition) come to be known as the *Anyathian* culture. It is a Palaeolithic culture consisting of what Movius terms 'choppers' and 'chopping tools', but is said to be entirely devoid of hand-axes of the coup-de-poing or Abbevillian type. Further, similar cultures have been reported from North-west India—the *Soan*, from North China—the *Choukoutienian*, from Malaya—the *Tampanian*, and from Java—the *Patjitanian*. Everywhere these cultures are apparently of the same antiquity, dating from the Middle Pleistocene and often persisting in a modified form into the Upper Pleistocene. Thus these widely separated regions appear to suggest the development over most of Asia of a great 'chopper-chopping tool complex' which paralleled

² It is important for the anthropologist to note that the correlation of the Villafranchian with Lower Pleistocene is vigorously contested by many European geologists of sound repute. Reference should be made to Dr F. E. Zeuner's recent monograph on 'The Pleistocene Period' (published by the Ray Society).

the hand-axe complex in Europe, Africa and Southern India, but which in fact developed quite independently. Whether the contrast between these two Palaeolithic cultures is really so distinctive as to demand the assumption of an independent and parallel development is a matter for the 'typologist' to settle. We may here note two points which perhaps tend to lessen the force of the arguments supporting such a distinction. One is that in the Northern Punjab a typical Lower Palaeolithic hand-axe complex is actually found in the same deposits (of Second Interglacial date) in association with the Early Soan chopper-chopping tool culture. The other is that the Patjitanian of Java does in fact include hand-axes, though the latter occur in relatively small numbers (only 6.2 per cent of the total series) and are said to show certain distinctive features suggesting an independent local development. In any case, however, we would demur from the view that the chopper-chopping tool culture is sufficiently distinctive and specialized to warrant the suggestion that it was developed in Southern and Eastern Asia by men of a physical type different from that found elsewhere. It is true that remains of *Pithecanthropus* have not yet been found in the 'hand-axe areas' of the world (with the possible exception of the fragmentary skull found in Africa on the shores of Lake Nyasa in 1935 by Kohl-Larsen); but there is good reason to suppose that this fossil genus represents a generalized ancestral type from which *Homo sapiens* everywhere was initially derived.

Remains of early man in the Far East have so far been reported from North China and Java. In North China, all the remains of *Sinanthropus* (or *Pithecanthropus pekinensis* as it may more appropriately be called) have come from one area (Locality 1) in Choukoutien. The age of the deposits in which the fossils were found have been tentatively correlated with the second Interglacial Period of Kashmir, though they may actually be somewhat older. Faunistic evidence suggests that the deposits were laid down at a time when the climate was milder and damper than it is today. It is interesting to note, by the way, that 70 per cent of the animal remains found in Locality 1 belong to deer, from which it may be inferred that 'Pekin Man' was a great deer hunter. There are also numerous hearths showing that he knew how to keep himself warm in inclement weather, as well as charred bones of animals bearing witness to his culinary activities. But he was, it seems, a mixed feeder, for the presence in the deposits of cracked shells of Hackberry seeds shows that he supplemented a meat diet with this shrub. He must, also, have been fairly skilful with his hands, for he developed a specialized quartz industry, and quartz is by no means an easy material to manipulate.

In Java, as already mentioned, it is extremely difficult on purely geological evidence to date deposits in which remains of early man have been found. Reliance must be placed mainly on faunistic correlations, and the latter give reason to infer that the Trinil deposits, in which the original specimen of *Pithecanthropus* was found half a century ago, are of Middle Pleistocene date, that is, a good deal later than has usually been supposed. Among the Trinil deposits, also, is a plant bed containing a flora typical of that which is found at the present day in Java 3-4000 feet above sea level—suggesting a climate 6-8° cooler. According to de Terra, this plant bed is to be correlated with the second glaciation of the Asiatic mainland. *Pithecanthropus* of Java thus lived somewhat earlier than 'Pekin man', and in relation to this it is interesting to note that the Javanese representative of this extinct genus was in some respects more primitive than the Chinese representative, e.g., in the smaller average size of his cranial capacity.

But a much older human fossil was unearthed in Java in 1936 at Modjokerto (near Soembertengah, towards the eastern end of the island). This is an infant's skull which,

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it is generally inferred, belonged to a baby *Pithecanthropus*. There appears to be no doubt about the stratigraphical horizon of this fossil—it definitely underlies the Trinil beds. Associated with the Modjokerto skull were found a number of animal remains representing the so-called Djetis fauna (e.g. *Epimachairodus*, *Nestoritherium*, and *Leptobos*), which suggests a correlation with the Villafranchian fauna of the Upper Siwalik horizon of India and thus with the First Interglacial Period (i.e. Lower Pleistocene).

At a much later date—long after *Pithecanthropus*—there came to Java a Neanderthaloid race of man, *Homo soloensis*, represented by a series of skulls discovered at Ngandong in Central Java. These people, it is suggested, migrated into Java when that island had direct connexion with the Asiatic mainland during the Third Glaciation of the Himalayas.

Readers of this brief review of Pleistocene chronology in the Far East will naturally assume that the glaciations of Asia were contemporaneous with the corresponding glaciations of Penck and Bruckner's terminology in Europe, and, indeed, this is probably a correct assumption if we accept the hypothesis that the Pleistocene glaciations were world events associated with fluctuations in the amount of solar radiation received by the earth (as postulated by Milankovitch in 1920). Nevertheless, it will remain an assumption, and not an established fact, until a great deal more laborious work has been completed by geologists and archaeologists in East Asia. The author of this illuminating treatise on Pleistocene Stratigraphy in East Asia is well aware of that. Indeed, he suggests the need for a very cautious approach to the subject of his thesis by the warning that, before any broad-scale correlations are attempted between Asia and Europe, it is a vital necessity that sequences should first be established in some intermediate regions such as Central Asia and the Near East.

A Find of the Early Iron Age from Llyn Cerrig Bach, Anglesey

by W. F. GRIMES

A FIND OF THE EARLY IRON AGE FROM LLYN CERRIG BACH, ANGLESEY (*Interim Report*). By Sir Cyril Fox. pp. 72 with Frontispiece and 26 plates at end. Cardiff (National Museum of Wales), 1945. 7s. 6d.

IN this publication the Director of the National Museum of Wales describes in detail a highly important find of Early Iron Age objects, made on a remote site in Anglesey in 1943 during wartime building operations. As its significance deserved, the discovery was given a good press at the time and readers of *ANTIQUITY* will already be familiar with it from the article reprinted in the June, 1944, number. But the title records that this too is an interim report: the final account, in which presumably will appear additional objects, one at least of which is of astounding quality, will be awaited with interest.

When the profit-and-loss account of archaeological discovery and destruction during the second world war comes to be cast, the Llyn Cerrig find must figure high on the credit side; for while much has been lost, and with it details of the greatest scientific importance, that which survives forms one of the most exciting collections of examples of prehistoric craftsmanship to be made in recent years. In congratulating ourselves on this result it is not inappropriate to remember with gratitude the officials, here and elsewhere, whom too often service etiquette and security requirements have buried in anonymity. The Llyn Cerrig find belonged to that difficult class which is unexpected and unheralded by any kind of surface indications; and the importance of realising the significance of a discovery and of reporting it quickly to the appropriate authority will be self-evident. Anyone who has laboured on defence sites in wartime, and seen the problems and difficulties of the officials concerned, will appreciate what it means for a harassed engineer to give time and attention to seemingly meaningless finds of decayed metal, in the certain knowledge that when the archaeologists are called in he is adding to his own work and making a rod for his own back. Suppression of such discoveries there has undoubtedly been in various places; but the Clerk of Works at Llyn Cerrig Bach is one of many to whom the thanks of individual investigators and of all interested in antiquity are not less due because they were carrying out an agreed official policy. There are of course gaps in the record of this series, for which in the circumstances no one can be held responsible. There must have been losses of objects which would no doubt have included some perishables that would have been of great value to us; and there has been no account taken of some of the more scientific aspects of the subject, such as the relationship of the finds to the peat-deposits. The reasons for all this will be obvious from Sir Cyril Fox's account; the fact remains that here we have a model report for others to follow, though they be not backed by the enlightened policy and the resources of the National Museum of Wales.

The report follows the now common practice of separating text and discussion from the catalogue, although the catalogue itself contains much—lists of types, some

discussion of details and analogies—that is more than mere cataloguing. Throughout Fox's descriptive prose is at its best : at once terse and varied, pleasant and easy to read for expert and layman alike : he has covered the full breadth of the related archaeological problems. This indeed is a feature that other travellers in the border countries of art and archaeology will do well to note. Pre-occupation with style and *motif* may so easily lead to forgetfulness of the fact that 'works of art' of this character cannot without danger be studied away from their context : the weaknesses of the purely aesthetic-morphological approach are to the archaeologist obvious enough. Fox shows himself sensible of the aesthetic qualities of his material, and he is completely aware of the artistic problems of the time. But his approach is essentially archaeological in the widest sense.

Here as we should expect his use of maps is valuable and illuminating. The reliability of the results of distributional study increases of course with the size of the group plotted : the smaller the number of objects the greater the generalisation. The writer has presented us with the distributions of individual types which taken singly could not be said to mean very much because they are not in sufficient numerical strength. But the cumulative effect of a series of such maps when there are sufficient types is overwhelming, especially when summarized graphically as in fig. 20. Here too the use of arrows to indicate trends of movement has certain dangers which can only be avoided by the constant reminder that they also are generalisations. Fox's fig. 20 is a striking, and as far as I know an entirely new, use of a technique which hitherto has been applied only to more general cultural or economic trends.

Of the material itself, Fox is clearly right in his conclusion, based alike on aesthetic and on chronological considerations, that the Llyn Cerrig *cache* is not the product of a single school of craftsmen or of one workshop. One wonders whether even in the bridle-bits there is as complete unity as he is inclined to suggest. The bits numbered 50 and 51 are first-rate examples of their type ; but of the superior refinement of 50 there can be no doubt. The crude central link which is an ancient replacement of the original mars its present appearance but cannot detract from the quality of the side-links with their well-modulated curves, their carefully balanced parts, and in particular the heart-shaped loops to which Fox rightly applies the adjective 'lovely'. It seems to me to betoken a more skilled hand than any of the other bits in the collection, good as they are ; in its original complete state it must have been one of the finest pieces of harness in the country.

But the *pièce-de-resistance* of the find as at present published is the crescentic plate, no. 75. Not only does it surpass in artistic quality the more utilitarian objects ; it has, as Fox shows, a profound bearing on the study of contemporary art-forms, and in particular of the 'mirror-style' which is the outstanding achievement of the pagan Celtic metal-worker in this country. Fox's promised analysis of the style in the light of this discovery deprives the impressions of a reviewer who glances at the subject in passing of any particular significance ; but his demonstration that the Llyn Cerrig triquetra is the coherent predecessor of the designs on some at any rate of the mirrors is ingenious and convincing, and would seem to suggest that the most profitable approach to an understanding of the decorative schemes of the mirrors is likely to be through the roundels which in the majority of the surviving mirrors are the important units in the pattern.

It is the concluding discussion which is most hampered by difficulties and gaps arising from the circumstances in which the objects were found. The problem is how to explain the presence here of such a collection, which seems to draw for its sources from every part of prehistoric Iron Age Britain, excepting only the more extreme north. Fox

concludes that the main source of the group was the area of the south-western B culture, whose pre-eminence in the later stages of the prehistoric development was, as he notes, almost certainly due to the combined presence of Cornish tin, Irish sea-traded copper and Forest of Dean iron. But here once again we are hindered by that outstanding *lacuna* in our knowledge of the prehistoric Iron Age: we know practically nothing of the north-eastern Iron Age B, in relation either to its settlements or to its distribution; and until something is done to fill this gap all discussion of the subject labours under a serious handicap—the more so because there is every indication that north-eastern B was an important element in the earlier British development.

But if all these relationships were clear the reasons for the presence of these objects in this spot, remote no less in ancient than in modern times, would still (in the absence of skilled excavation of the site) remain obscure. The writer is content to state the alternatives, though his own preference appears to be for the Druids. As he truly says, the cult is discredited in the eyes of serious workers nowadays; but in such a place and period the question of a connexion with a Druidical centre cannot be lightly dismissed.

And finally a word about the illustrations, which are an important element of such a publication. The National Museum has treated us generously and the pictorial record is very complete. Mr Waterhouse's drawings impress as being generally careful and accurate, although his surface-treatments show a certain sameness which I suspect is not shared by all the objects drawn. (This question of surface-rendering seems to me to involve a matter of principle which archaeological draughtsmen would do well to consider more carefully.) Two of the drawings however fall below the standard of the rest: the perspective of the nave-hoops, Pl. VII, is not true (and surely, by comparison with Pl. XXI they have been much furbished up?); and Pl. XVIII, the crescentic plaque, is not as sensitive a rendering as this admittedly difficult subject deserves. Many of the lines on this drawing must indeed be regarded as completely meaningless: comparison with Pls. I and XXVIIIa will show how far they are from capturing the lovely texture of the original bronze.

But I have no wish to end on a critical note: these are minor points which do not detract in any way from a publication which is a valuable contribution to the archaeology of the Iron Age. The final report, I repeat, will be awaited with interest.

Vortigern and Aetius—a re-appraisal

by P. K. JOHNSTONE

VORTIGERN of Gloucester (1) is perhaps the most maligned figure of British history. There can be little doubt that his reign ended in widespread disaster, but that very fact may have made him a traditional scapegoat. Some at least of the charges against him can readily be disproved—for instance, he certainly was *not* the father of the British-born Gallic Bishop, Faustus, by his daughter or anyone else. Faustus and Vortigern must have been born within a very few years of each other (2). Perhaps the Gildasian epithet of *infaustus tyrannus*, applied to Vortigern, served as a starting-point.

Vortigern is also styled a weak ruler. This is inherently improbable. Weak men do not win and hold power in an age of breakdown and confusion. Even Gildas, who detested his memory, admits that Vortigern's harsh rule gave sub-Roman Britain a period of prosperity (3). Vortigern may well have been brutal and short-sighted, but he was first the winner of a struggle for survival in which no holds were barred.

It is usual to call him a rough Welsh hill-chieftain. But if his Roman-named ancestors were magistrates of Glevum Colonia he is not likely to have been an untutored backwoodsman. That he is remembered by a purely Keltic name may be more or less an accident. As an aristocrat he probably was given more than one name. At any rate he gave one of his sons a Latin name, Pascentius, which probably implies that his family was Christian. His first wife, Severa (?) may have been a descendant of the Emperor Magnus Maximus. All this suggests a Romano-British nobleman of large ambitions (4).

But of course the main charge against Vortigern is that he 'brought in the Saxons'. Tradition is unanimous that it was he who employed the North Swabian brothers, Hengist and Horsa (5) and gave them their first foothold in Thanet. Yet if the accepted date of c. 449 is even approximately right, this came several years after a probably contemporary Gallic annalist noted that Britain, once Roman, had fallen under Saxon rule (6). And two full decades earlier St. Germanus had found Saxons invading the North Midlands and defeated them, perhaps in Derbyshire (7). Thus, either Vortigern's reign has been post-dated by over twenty years, or else he rose to power in a Britain already seriously affected by 'Saxon' invasion. I am inclined to accept the usual dating, not so much because of Gildas' narrative as because it is so difficult to reconcile

¹ *Historia Brittonum*, cap. 49. That Vortigern's pedigree is traced to the eponymus of Gloucester would imply his family's long connexion with the *civitas* of Glevum. This does not necessarily mean that his ancestors were pre-Roman kings.

² Baring-Gould and Fisher, *Lives of the British Saints*, II, art. S. Faustus.

³ Collingwood and Myres, *Roman Britain*, p. 315.

⁴ See the pedigrees collected by A. Anscombe in *Y Cymmrodor*, xxv, 75, and xxix, 151. The probable source of 'Severa' is the Valle Crucis Pillar.

⁵ Hodgkin, *History of the Anglo-Saxons*, II, 720.

⁶ *Monumenta Germaniae Historiae*, auct. ant. IX, 660, 661.

⁷ A hilly region, North of the Fens, is a logical deduction from the *Vita Germani*. But it may have been in the Chilterns.

the pedigrees of descendants of Vortigern and his contemporaries (including those of Hengist) with the earlier date, according to which the 'Saxon Advent' took place *c.* 428. For instance; the birth of St. David can be placed at *c.* 512 with a fair degree of confidence. His mother, St. Nonnita, was probably born *c.* 490. Her parents were Cynyr Caergawch, a Demetian *subregulus*, and Anna, daughter of Vortimer. We can hardly place Anna's birth much earlier than *c.* 455, and Vortigern seems to have survived his eldest son. These pedigrees are often regarded with great scepticism, and certainly they are subject to blunders of many sorts. But their coherence and agreement are very impressive (8).

If we could be quite sure that Bede's Kentish informants were correct in stating that Hengist's son and successor was named Oeric, the 449 dating would have another prop. For Oeric is almost certainly a namesake of the great Visigothic king, Euric (457-484). Unfortunately, by one of those maddening coincidences, an error on the part of some copyist makes it quite uncertain whether Oeric was Hengist's son or grandson. But all versions of the Kentish royal pedigree make king Aethelbert (born 552? ob. 616), the great-great-grandson of Hengist. If we place the *floruit* of Aethelbert *c.* 585, even at 35 years per generation, it is hard to push Hengist back farther than *c.* 445 (9).

All this fits in very well with a passage in the *Historia Brittonum* (10) implying an interval of forty years between 'the termination of the Roman power in Britain' and the ascendancy of Vortigern. This points to *c.* 447. On the other hand we have a statement that Hengist and Horsa arrived in the fourth year of Vortigern's 'reign' (11). Four years counted backward from 449 would give us 445-6. So small a discrepancy may be considered substantial confirmation, and we seem justified in placing the accession of Vortigern between 445 and 447.

The precise steps by which Vortigern gained his supremacy remain obscure. The detailed narrative given by Geoffrey of Monmouth (12) does not suggest any solid basis of tradition. But some of the factors involved are clear. If, as early as 441-2 Saxon ravagers were driving large numbers of Romano-Britons overseas to Gaul, convinced that the entire province was doomed, it is evident that the Romanized south-east presumably dominant in British affairs since 407, must have been seriously weakened. But Vortigern's patrimony, the canton of the Dobuni, lay well outside the regions most exposed to Saxon attack. He thus may have been able to offer his assistance to the harassed citizens of London, Verulam and Lincoln—at a price. At a much earlier period he had probably gained the allegiance of the South Welsh, including the Irish Desi settled in Pembroke (13). The 'king' of Worcester was his vassal (14). The forces at his disposal must have been considerable.

Gildas (15) implies a British victory of some consequence, *c.* 447, over 'enemies who had for so many years been living in their country'. He implies that these enemies were Picts and Scots, but if his chronology is even approximately correct, they are much more likely to have been Saxons—possibly with Pictish allies as in 429. Since this

⁸ The best sources for the Welsh genealogies are Anscombe (op. cit.), Baring-Gould and Fisher, op. cit. vol. I, and E. W. B. Nicholson, *The Dynasty of Cunedag*, in *Y Cymmrodor*, XXII. For a critical discussion, see Chadwick, *The Growth of Lit.* I, 309.

⁹ Bede, II, c. 5. *Historia Brittonum*, c. 58.

¹⁰ H.B., c. 31.

¹¹ H.B., c. 66 (computus).

¹² H.R.B., Book VI, cc. VI-X.

¹³ The Irish Bishop, Fortchern of Trim, baptized by a follower of Patrick, before 447, is a Goidelicized namesake of Vortigern.

¹⁴ H.B., c. 37. Cair Guoyrangon is Worcester, ib. c. 7.

¹⁵ cc. 20, 21.

victory must have preceded or followed Vortigern's accession very closely, it is hard to deny him at least a share in its credit. Much overrun territory was regained. 'The enemy left our people . . . the audacious invaders return to their winter quarters, determined before long again to return and plunder'.

What districts were overrun in 441-7? Without being too dogmatic, one is inclined to point toward the regions bordering on the Wash and the Humber. It may have been during these years that the 'beautiful silver flagons and dishes . . . evidently loot from a church and from the mansion of some wealthy landowner (16)' went north from—perhaps York—to the (presumably) Pictish hill-top town of Traprain Law. The 'house . . . inhabited till about 400' inside the walls of Caistor-by-Norwich may have been then burnt over the heads of its thirty-six desperate defenders (17). Some of the 'Luton type' brooches from Bedfordshire could very well be this early (18). The old-fashioned warrior of Dorchester-on-Thames might indicate the deepest penetration, before the British rally (19). If the Midlands, from (say) Yorkshire down to the Thames had been pillaged, burnt, overrun and terrorized by Saxon bands, it is easy to understand how those who fled across the Channel felt sure that Britain was now a Saxon land. We are only beginning to realize how hard—and how incompletely—Roman Britain died.

Did the British recover all their lost territory? Gildas does not say so. The 'winter quarters' may have been no farther than Norfolk or Yorkshire. The Anglian chief, Soemel, remembered by the kings of Deira as their first ancestor to settle on British soil (20) can scarcely be dated much later. But for a time the pressure was relieved. The surviving cities had a breathing-spell.

Three events were more or less coincidental with Vortigern's reign; (a) the revival of Pelagian preaching in Britain, (b) the second visit of St. Germanus, and (c) the Appeal to Aetius. Perhaps the combination is not merely coincidence.

If Vortigern and his chief followers were Pelagians they may well have encouraged the followers of Pelagius, disowned by the British Church in 429, to again become active. To a man who knew himself to be in the eyes of the Roman government a *tyrannus*, a British Church completely out of touch with Rome may have appeared highly desirable. The legend of Vortigern's antagonism to St. Germanus must have had a starting-point somewhere—nowhere more likely than on the question of religion.

But Vortigern's eldest son, Vortimer, was old enough to take an independent line of his own. In Welsh tradition he is always 'Guorthemir the Blessed'—Blessed by whom? By St. Germanus, naturally (21). Vortimer's 'conversion' to orthodoxy can hardly have failed to produce something verging on a rift with his father. The Synod of 447 (? at Verulam) was a clear-cut triumph for Germanus. It may have damaged Vortigern's prestige materially. But it left him still secular overlord of south Britain.

The 'Groans of the Britons' to 'Aetius, thrice Consul' is usually dated 446, the year of Aetius' third consulate. Actually, it might have been written as late as 453 (22).

¹⁶ Hodgkin, I, 58. ¹⁷ *ibid.* I, 53. Collingwood and Myres, 302. ¹⁸ Hodgkin, I, 113.

¹⁹ Hodgkin, I, 109. Collingwood and Myres, 394.

²⁰ D. Haigh, *The Conquest of Britain by the Saxons* (1861), 133. E. Foord, *The Last Age of Roman Britain* (1925), 142, 161.

²¹ Haigh, *op. cit.*, 230.

²² Aetius was Consul for a fourth time in 454, and during his consulship was murdered by his puppet-Emperor, Valentinian III.

But in 447 the Britons were in close touch with a Gallo-Roman statesman of the first rank, who, when he left Britain, went on to the Imperial Court at Ravenna—a court dominated by Aetius. What more reliable messenger could the ‘Roman’ Britons have found than Germanus? The ‘Groans of the Britons’ was clearly a document composed in the purple, ‘Hisperic’ style. If this were not so, one might ask whether the reference to the sea driving the Britons back to their barbarian foes is not evidence of a disastrous subsidence of the British coast. But it is probably only rhetoric. Two facts do emerge. If we are right in our interpretation, the *barbari* were Saxons. And the men who wrote the appeal to Aetius were frankly pleading for Roman intervention. They were confessing their inability to cope with the situation ‘. . . the barbarians drive us to the sea. The sea drives us back . . . We are either slain or drowned (23)’. These are not the words of a Vortigern rising to power, but of the philo-Roman magnates of southeast Britain, demoralized by Saxon savagery, but bitterly resentful of the heretical usurper from the West who had temporarily delivered them, and deliberately mistrustful of his continued protection. They were evidently well-informed as to Roman politics. They address not the nominal Emperor Valentinian, but his domineering *Magister militum*, Aetius, the actual ruler of the Western Empire since 433. If Britain was to be rescued from the Saxon—and from Vortigern—it would be Aetius who would make the decision.

Of course the story of the appeal leaked out. Even today, such facts have a tendency to rise to the surface despite all precautions. Most late-Roman rulers had some sort of Intelligence Service, and Vortigern was not the man to be an exception. Sooner or later, he came to know that the appeal was in Aetius’ hands—and Aetius’ hands were strong. Of course he had not acted immediately. There was no reason for anyone to expect him to do so. Such things need preparation. When Aetius moved, it would be too late for Vortigern to improvise. When the *Historia Brittonum* mentions ‘Roman invasion’ among the fears of Vortigern (24) it is only confirming an already reasonable inference.

One precaution the *superbus tyrannus* could take immediately. His satellite *Veranconus of Worcester became king of Kent—no doubt to the indignation of the Kentish nobles who became his subjects. But Vortigern could not assure himself that this was enough. What he needed was a force—preferably a naval force—that could be counted on not to desert to Aetius. From this standpoint, no British force was absolutely dependable. ‘Rome’ was a magic name, even to the hill-top kinglets of the Keltic West. To Vortigern the conclusion must have been inescapable. He must enlist mercenaries.

But mercenaries came high. Vortigern could not long maintain a large force from his own income. The British *civitates* must be persuaded to contribute. Now nothing could have been farther from their wishes than to be taxed to maintain foreign troops to fight off Aetius, whose help many of them had implored. If the proposition were put to them in plain terms, they could be depended on to refuse. Vortigern’s power was not absolute. This was not so much because of any ‘constitutional’ limitations, written or unwritten, but rather due to the fact that his power rested on a loose network of allegiances which might fall apart at any time if strained too far. In this respect the British ruler was very much like a later Bretwalda, or an Irish high-king.

Vortigern’s ingenuity was equal to the test. Soon ‘a vague rumour, suddenly as if on wings reaches the ears of all, that their inveterate foes were rapidly approaching to

²³ Gildas, c. 20.

²⁴ H.B., c. 31.

destroy the whole country, and to take possession of it, as of old, from one end to the other (25) . . . ' It is evident that the Ministry of Propaganda was efficiently filled at this period.

It is not necessary to suppose that these reports were wholly fictitious. After four years of quiescence, the Saxons defeated *c.* 446-7 must have been stirring again. All that Vortigern needed to do was to see that all reports of Saxon or Pictish activity were given full circulation. If the rumours had been utterly unfounded, they probably would not have deceived the British council (? a survival of the provincial Senate) which is hardly likely to have been notable for simple faith.

When the necessary funds had been voted, Vortigern was able to act swiftly. He was probably already in touch with the three-ship fleet commanded by a certain Hengist, who represented himself (we still do not know how truly) as a homeless exile. Such men and their followers must have been common enough along the Saxon Shore. But Hengist was the grandson of Witta, king of the Swaefe (26)—the North Suebi. The Swaefe were neighbours and (apparently) close allies of Angles, still dwelling in 'Ongul', between the Eider and the Baltic. Now Hengist's brother was Horsa (the shortened form of a name such as *Horstheof or *Horswalda) and in the western (presumably Suebic) part of Schleswig Horsted still bears his name (27). This looks very much as if Horsa was a reigning monarch among the Angles—probably a *subregulus* of king Eomaer, grandson of Offa the Gentle (28). Hengist, if not himself English, certainly knew all the Angles (29).

How much of this Hengist told Vortigern, we can only guess. He may have made use of these and similar facts, quite truthfully, to convince the Briton that, though he was a 'Saxon' in the wider sense (*i.e.* a seafaring Teuton) he was quite distinct from the Saxons recently troubling Britain. The two men seem to have found each other quite agreeable. Hengist was commissioned to act both as commander and recruiting officer of Vortigern's Foreign Legion. If Vortigern stressed the possibility of campaigns against the Picts and did not bother to mention the nearer Saxons, it is only what we should expect. Both for the Britains who paid the money and the—dare we call them Jutes?—who would do the fighting, it was pleasanter to stress the prospect of warfare against the 'Picts and Scots'.

Approached from this angle, the settlement in Thanet, so puzzling on the usual view (30) becomes easily understandable. It was certainly convenient as a recruiting-office. The island may have long been a neutral ground, where Britons might ransom their kinsfolk from pirates anxious for a quick turnover. But above and beyond all this, it was the perfect naval base from which to oppose a cross-channel invasion. And in Gaul (450-1) Aetius was still mustering his forces. Perhaps Vortigern heard that he was ready to fight Attila the Hun. In which case he no doubt reflected, ' . . . that is just what I would say '.

²⁵ Gildas, *c.* 22.

²⁶ K. Malone, *Widsith*, 189, 197.

²⁷ See the map in Hodgkin, I, 5.

²⁸ G. P. Baker, *The Fighting Kings of Wessex* (1931), 46. H.M. Chadwick, *The Origin of the English Nation* (1906) *passim*.

²⁹ He is later attended by 'elders of the Ongul (?) race', H.B., *c.* 37.

³⁰ Hodgkin, I, 78.

The Sutton Hoo Shield*

by HERBERT MAYON

FOR its importance in throwing light upon a dark period of history, and for the intrinsic beauty and archaeological significance of its contents the Sutton Hoo Ship burial has been rightly described as the greatest find ever made in England. The Ship was fully excavated in July and August 1939, and the treasures found therein were taken to the British Museum, where the most urgent preservative measures were immediately undertaken. During the War these valuable finds were hidden underground. Recently they were returned to the British Museum, where they are now being unpacked, cleaned, and made ready for exhibition. In anticipation of their full publication it has been decided to make a report upon some individual pieces when, from time to time, they become available for exhibition.

The great Shield claims a place among the most important works found in the ship, and the illustrations demonstrate the amount, and kind of work, which had to be done to it in the laboratory, in preparation for its public exhibition.

Let us imagine that we were watching the excavation of the Ship, and observed the emergence of the great iron boss, which proved that a shield once lay here (FIG. 1). Thirteen hundred years underground had reduced its woodwork to fragments. Around the boss lay metal fittings, broken and corroded. Pieces of gold foil were scattered in the earth. Here and there were seen traces of fragile gesso, and the occasional gleam of a garnet inlay. Near the boss lay the bird's head and leg, the dragon, and the gilt bosses from the front of the shield. We should recognize the skill of the excavators. From various angles they took photographs of the objects as they lay in the earth. Then with infinite care they gathered them up and packed them for transit to the Museum. Those photographs have proved themselves of immense value in the interpretation and re-assembly of the work.

For example, it was a study of some of them which led Dr Plenderleith to pick out from among the fragments of bronze and iron those pieces which made up the shield-grip (FIG. 2), which, with its elaborate cast bronze extension at either end, makes so decorative a feature on the back of the shield. The length of the shield grip is 25 inches. The grip itself is of iron and extends for a distance of some seven inches in either direction from the centre of the shield. Both ends of this plate are covered by the decorative terminals. These are of gilt-bronze with tinned and niello decoration, and garnet inlays.

The presence, at one end, of a little group of bronze fittings clearly belonging to the shield, showed that it had extended rather further in that direction. Actually we arrived at a measurement of 33 inches for the maximum length of the shield. But what was its real shape? Could it have been of sub-rectangular form like the Battersea and Witham shields? The fragments as they lay in the ground seemed to suggest a straight-sided shield. But then we reflected that such shields belonged to a period 500 years too early. The known seventh century shields seemed always to have been circular

* We are asked to state that the views expressed in this article are those of the writer, and must not be regarded as representing the official views of the British Museum.

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FIG. 3. PLAN OF BURIAL CHAMBER

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in form. For example, the shields shown on the Franks Casket, a seventh century work, were round. So also were the Scandinavian shields of the period. Could the Sutton Hoo shield have been circular too? Could its sides have been driven inwards while the shield lay in the earth?

Consideration of the plan (FIG. 3) showed that two heavy objects—the 'standard' and the whetstone—lay just alongside the shield, and all three lay close to the western end of the burial chamber, which, after standing for many years, had collapsed inwards under the pressure of the sand piled against it. It seemed probable that if, by that time, the thin wooden shield had become decayed, the collapse of the wall would have been likely to drive the edge of the shield inwards in the direction of its centre line. A further examination of the photographs enabled us to observe this movement, as it were, actually in progress (FIG. 1). The weight of the shield-boss had caused it to sink through the decayed wood of the shield, so that the shield-border, a ring of wood rather more than two inches wide and a little thicker than that of the adjacent parts of the shield, had broken loose and slid forward till it had in fact overhung the wide flange of the shield-boss. As it moved forward it split up into numerous narrow sections, and

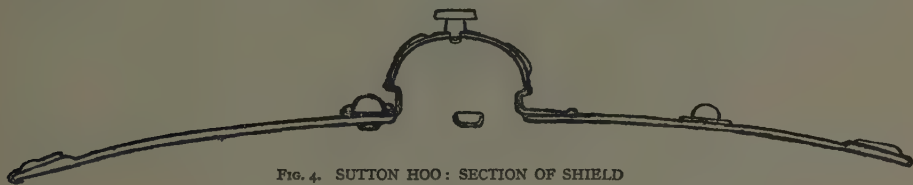


FIG. 4. SUTTON HOO: SECTION OF SHIELD

the photographs show some of these sections moving a little in advance of their neighbours. We found also that the edge of the shield had been strengthened by a u-sectioned bronze binding made from a folded half-inch strip of bronze. This had run right round the shield, protecting and stiffening the edge. This band, decayed and fragmentary though it was, had done something to keep together the broken parts of the edge of the shield, so that they had moved forward more or less as one. Further pieces of evidence convinced us, at last, that the shield had been circular. I give an example of what I mean. The u-shaped band round the shield had been of bronze, covered with gold leaf. A number of short lengths, protected by the gold, remained intact. They were seen to be curved. We drew out on the bench parts of a number of circles of about the same curvature and tried the lengths of bronze against them. At $16\frac{1}{2}$ inches radius, or 33 inches diameter, they fitted exactly. Thus our original estimate for the diameter of the shield was confirmed.

The next question to consider was its curvature. Was the shield flat or was it slightly curved, like a watch-glass? We were fortunate enough to find a complete answer to this question, for the shield-grip, the strip of metal on the back of the shield, with its elaborately decorated extensions at either end, gave us what we wanted (FIGS. 2 and 4). It was broken into half a dozen pieces with clean fractures. These pieces we stood up on edge on strips of plasticine. It was evident at once that they formed a curve: a curve which must have corresponded with that of the back of the shield. So that question was settled without difficulty. Next we considered the shield's thickness. Among the objects recovered from the area where the shield lay were three gilded, hemispherical, metal bosses, each about $1\frac{1}{4}$ inches in diameter, with an ornamental border beyond. Fastened beneath two of these bosses was a metal pyramid—the third boss

had only a flattened rivet. Figure 5 shows one of those provided with a pyramid. On examination the central portion was found to be composed of a layer of wood, five-sixteenths thick, with a sheet of leather on either side, and a portion of a decorative leather strap on the inner side, next to the pyramid. It was a complete section of the shield. From their position in the earth the two pyramids evidently held the strap through which the warrior passed his arm when about to seize the hand-grip of the shield. We were able to confirm this observation by consideration of the direction of the grain of the wood in relation to the remains of the strap. Here grain of the wood ran horizontally, as elsewhere in the shield, and the strap ran vertically. So, if the strap ran between them the two pyramids must have been placed one above the other.

We now come to the great central boss. FIGURE 6 shows the boss before cleaning, and FIGURE 7 the whole Shield with the boss in position. It is of iron, with tin, gilt bronze, niello and garnet decorations. The central domed portion is nearly $5\frac{1}{2}$ inches diameter outside, and $3\frac{3}{4}$ inches in height, with a border $1\frac{3}{4}$ inches wide running round its base. Projecting from the centre of the boss is a gilt bronze disc with zoomorphic ornament, garnet and niello decoration. A band with zoomorphic decoration surrounds the support for the projecting disc, and below this band five dragons' heads in gilt bronze run downwards over the surface of the main boss. In the intervals between them five more dragons' heads rise up from the lower part of the boss, partly filling the spaces between the first set. Below these heads a decorative necking of tinned bronze connects the boss with its wide ornamental border. On this border are five gilt-bronze bosses, each with a raised base-ring of tinned bronze. Panels of gilded zoomorphic ornament separate these bosses. A few words are necessary here on the question as to which way round the great shield boss, with its five subordinate bosses, should be placed on the shield. Should one minor boss come just above the main boss, towards the top of the shield? Or should one be turned round till it came just below the centre? On consideration of the different possible positions it became evident that there was only one way in which it could be placed satisfactorily. That was when one small boss came midway, opposite these two bosses, behind which the arm-strap ran. If we turn to the back of the shield (FIG. 2) it will be noticed that the hand-grip does not cross the opening into the shield-boss centrally. We had found traces of the rusty iron grip beside two of the subordinate bosses—not straight across the middle of the opening as we might have expected. We were curious at this unexpected placing, but found that there were precedents for it in some of the contemporary Scandinavian shields. However, it was another observation which settled the matter. If one passed one's arm through the arm-strap and attempted to seize the hand-grip, an asymmetric position for the hand-grip was found to be essential. For the back of the hand required far more room than that needed for the fingers, and there was not enough room for it with a centrally placed grip. Indeed, we found that the only comfortable position for the hand-grip was that originally devised, and reproduced in the restoration. Near the bottom of the shield, just beyond the extremity of the elaborate grip-ornaments, was a bronze fitting, evidently for the shield's suspension straps. Strap-ends, of tinned and gilded bronze, were found in the sand nearby. Fragments of a leather strap were found tied round the upper end of the iron grip.

The edge of the shield was protected by the U-sectioned binding of gilt bronze already described. It made a good finish to the edge, for it gripped within its open lips the wooden shield itself and its coverings of leather on either side. At intervals of about 9 inches all round this border were dragon heads, about 2 inches long, in gilt bronze—twelve in all. They projected towards the field of the shield, and each was

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fastened down by a bronze clip which passed round the edge of the shield and was riveted to it. On each side of every dragon's head, close to the rim, was a gilt-headed nail. Nine of the original twelve dragons' heads had been replaced in antiquity by copies in gilded gesso. In the restored shield all of them have been replaced in gilded metal.

On the dexter side of the shield, in front, is a gilt boss with raised border, forming the centre of a strip of zoomorphic ornament, in gold foil, and about $14\frac{1}{2}$ inches long (FIG. 8). This decorative feature is the aesthetic survival of one of those iron bands which were sometimes employed to bind together and strengthen a shield, and were frequently present in contemporary Scandinavian shields. They were placed at right angles to the grain of the wood, as here. This gold foil ornament fulfils no functional

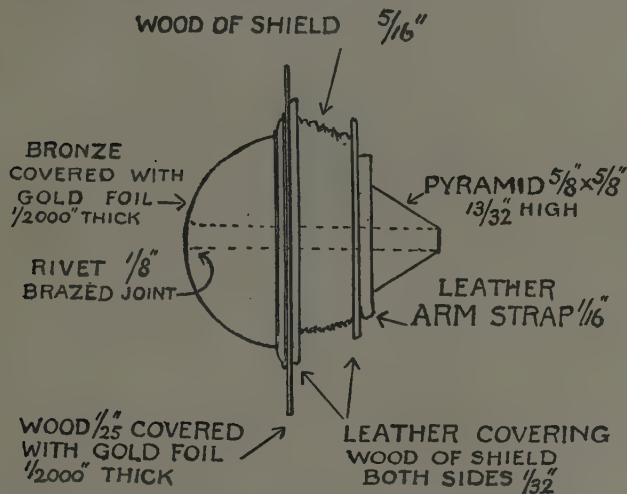


FIG. 5. SECTION OF SHIELD NEAR ARMSTRAP

purpose now, but it does something to balance the design of the shield as a whole. The gold foil is so fragmentary and in such a fragile condition that in the restoration of the shield it is replaced in gilded metal. It was considered to be rather a triumph to be able to reconstruct the original with certainty.

We now come to two most striking features in the decoration of the shield—the bird in the upper part, which we shall discuss later and this curious creature below (FIG. 9). For its significance it will be well if we turn for a few moments to the stories which centre round the great Scandinavian sixth century hero Beowulf, stories which give us so vivid a picture of the times and the peoples with which we are concerned. Beowulf was the nephew of Hygelac, ruler over one of those Gothic tribes who lived in the southern provinces of Sweden and the adjacent Baltic islands. Hygelac seems to have been an historic person, for Gregory of Tours writes of one Hugelc who led his fleet from the Baltic to the mouth of the Meuse or Rhine, but was defeated and slain by Theodebert, son of the Frankish King Theodoric. This was in 515 or 516. Beowulf, who was a mighty warrior, was a successor of Hygelac, and ruled the land wisely and successfully for many years. When he was growing old his subjects became very

concerned at the depredations of a fierce creature which is variously described as a dragon or as a serpent. In Scandinavia such mythical creatures are the usual guardians of hidden treasure. So we read here of 'the lair of the serpent, the ancient creature who flew by twilight to look at the treasure'. This particular serpent was especially dangerous, as his breath would burn flesh or armour. Beowulf, with the help of a faithful comrade destroyed the dragon, but lost his own life.

Many representations of the fight between Beowulf and the serpent have survived in Scandinavian art (FIG. 10). Here is one which shows the hero, riding on his horse, advancing towards his opponent. Knut Stjerna, the well-known Swedish archaeologist, pointed out that the figure on this little panel cannot represent the god Odin, for in Scandinavian mythology there is no mention of any contest between Odin and a dragon. The figure is that of Beowulf, a hero round whom the legends of the country became focussed. It is clear that the warrior for whom the Sutton Hoo shield was made was familiar with such stories, and put the dragon on his shield to indicate that he himself would be prepared to undertake such a quest. The dragon on the Sutton Hoo shield is a remarkable piece of work. It is of gilt bronze, richly chiselled, with tinned and niello decoration, and garnet inlays. The dragon is provided with four pairs of legs, or wings. The figure, having been damaged in antiquity, the last pair was then replaced in gilded gesso. A prolonged search through published records of Scandinavian and Teutonic finds has failed to produce any comparable figure. The bird in the upper part of the shield, again, has no peer. Its head is of gilt bronze, with garnet inlays. Its leg is of gilt and tinned bronze with inlays of garnet and glass. The wing was made of a thin piece of alder wood covered with gold foil, which was decorated with patterns in relief. Now it happened that the whole of the wood which formed the front part of the wing had survived, together with its gold covering, and an impression of the pattern on the gold was still visible on the surface of the wood on which it lay. We had no doubt at all therefore as to its correct restoration. But to complete the wing we still needed to know the position of the third corner (FIG. 11). We found that the bronze plate which formed the leg and foot of the bird had protected the surrounding parts of the wooden fabric of the shield, possibly through some chemical action of copper salts from the bronze, so that a piece of wood about three inches square, with the bronze leg still attached to it, had survived from that part of the shield. In the angle between the bird's leg and foot we found an impression of the remaining corner of the wing, and the orientation was determined by the direction of the grain of the wood to which the leg was still attached. Thus we were able to reconstruct its complete shape. The tail of the bird was formed of a small rectangular panel in gold foil with an elaborate pattern.

All that remained of the body of the bird were a few fragments of leather and fragile wood. A careful search of published material has produced no contemporary example of a similar bird, with head and leg in profile, and a wing such as we have here. The body of the bird on the shield is therefore a conjectural restoration.

It may be asked 'what is the significance of this device—a bird placed "in chief" on the shield'? Now from Scandinavian and other sources we learn that it was the custom of chieftains and warriors to bear on their shields, or helmets, or banners, or ships, some distinguishing badge. Thus Beowulf had on his banner a boar's head. He and his followers had on their helmets the figure of a wild boar. We read elsewhere of a raven banner, and so on. It is quite probable that the chieftain who was commemorated at Sutton Hoo would place upon his shield some, shall we call it, heraldic device. He chose this bird. We shall probably ask the question 'why did he choose

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it' ? Before we attempt to answer that question let us take a glance at the state of south east England from the middle of the sixth century onwards. South of the Humber from the year 560 Æthelbert, king of Kent, had been the most important ruler. He was the first Christian overlord of East Anglia, a district which had been entirely pagan till then. Redwald was its next ruler. He had taken East Anglia and Mercia from Northumbria. Redwald was a pagan at first, and, later, though nominally a Christian, he had retained two altars in his church, one a Christian altar, and alongside it one dedicated to the pagan gods. He held his court at Rendelsham in Suffolk, and died about 628. From the splendour of the objects recovered from the Sutton Hoo grave it was natural to conclude that the person commemorated was a king, and, with less material upon which to work than that now available, some writers concluded that the king must be Redwald. But we have seen that the evidence from the coins is against such an attribution, and there is other weighty evidence against it. Redwald's son, Eorpwald, was slain by a pagan, Richbert, soon after his accession to the throne, and again



FIG. 10. BRONZE PANEL FROM HELMET
VENDEL, GRAVE 1

the district was entirely pagan. The next king, Sigebert, a Christian, abdicated after a short reign. He and his two next successors, Ecgric and Anna, were all killed in wars with the pagans. Christianity could make little headway in such troublous times. But in 655 the supremacy passed to Oswiu, Christian king of Northumbria, and the final evangelization of East Anglia began.

A wholly pagan burial such as that at Sutton Hoo was quite possible at any time up to the year 655, and after that time it would become progressively less likely as Christian influence increased.

I would now refer to an event in the early history of Sweden which has a definite bearing upon our problem. By the beginning of the sixth century those Gothic tribes who inhabited the southern provinces of Sweden had become so interested in the recent dissolution of the Roman Empire that they had left their homelands comparatively weakly garrisoned. The Swedish king seized his opportunity, and made war upon the Gothic ruler. He boasted that he would kill his opponent and throw out his body to be devoured by crows. In the campaign which followed he himself was driven back to his stronghold at Vendel, a place in the province of Uppland, some fifty miles north of Stockholm. It was the Swedish king who was defeated there, and his body thrown to the crows. -In this way he gained the nickname 'Vendel-Crow'. Later, the Vendel

folk and their rulers perhaps became rather proud of that title 'Vendel-Crow', and it would seem that at least one of them adopted it as a badge of honour.

During the years from 1881 onwards, in that same village of Vendel a series of remarkable discoveries was made. Near the church a group of fourteen ship burials was found. The interments date from about the year 600 to after the year 1000. So remarkable were the finds from these graves that the whole period has been called 'The Vendel Age'. Other burials of a similar character have been found at Valsgärde and at other places in Sweden and at one place in Norway. The ship graves at Vendel were not those of the kings of Sweden or of Norway, but were those of local chieftains, who probably held some such position in the country as, say, the heads of the great Scottish clans did in Scotland down to the eighteenth century. The graves at Vendel seem to have belonged of a single family, and they are spaced out, at intervals of some thirty years, over the whole period of four hundred years.

The finds at Sutton Hoo exhibit many remarkable parallels to those of the Vendel Age. Here is a piece of ornament (FIG. 8) from the front of the Sutton Hoo shield, placed beside a similar piece of ornament from the front of one of the Vendel shields. It can be seen that in spite of some notable differences, the general design of the two pieces is similar. I would draw attention to one point of difference—the presence of grapes in the Sutton Hoo panel. Now grapes, as an artistic motif, are rare in Scandinavia—their home is farther south. I shall refer to this point again. On the great shield-boss from Sutton Hoo (FIG. 7), there are five bosses round the flange, with zoomorphic panels between each. The dragons' heads descend from the centre, between other dragons' heads rising above the necking. A shield-boss from Vendel (FIG. 12) shows how close is its resemblance to the one from Sutton Hoo, and there are scores of parallels in Sweden from the Vendel Age and those found at Sutton Hoo. This similarity is not confined to the shield and helmet alone. It is present throughout the whole find, including the jewellery, with the exception of those imported works like the great Byzantine dish, the salver and the silver bowls, the Coptic bowl, the coins, and of course, the hanging bowl with escutcheons, which probably had never left our shores. We feel that at Sutton Hoo it is the work of a single school or workshop which may be felt throughout. We cannot believe that the helmet and shield were produced in Sweden, the jewellery in another place. With the exceptions I have indicated, the whole find holds together as one. This is an important new fact, a critical piece of evidence. It supports our conviction that practically the whole of the Sutton Hoo ship treasure is an importation from the Uppland province of Sweden. The great bulk of the work was produced in Sweden itself. It is neither East Anglian, Anglo-Saxon, nor Jutish, though it is quite possible that the craftsmen who were responsible for the jewellery received their training at some centre from whence the Jutish craftsmen of Kent received theirs.

Though much of the work from Sutton Hoo is contemporary with that in the earliest of the Vendel graves it exhibits some notable differences from every other find of that time. I enumerate a few :—

- (1) on no other shield do devices appear which are comparable with the bird, the dragon, and the dragon-head rim of the shield from Sutton Hoo. In the other Scandinavian shields a raised rim and a few bosses or bars seem to provide sufficient decoration, apart, of course, from the shield boss itself ;
- (2) a prolonged search through Scandinavian and Teutonic material has failed, hitherto, to discover any comparable bird—with head and leg in profile, and a wing such as we have here ;



FIG. 1. SUTTON HOO: THE SHIELD BOSS AND FITTINGS EMERGING FROM THE EARTH

PLATE II



FIG. 2. THE SUTTON HOO SHIELD: REVERSE



FIG. 7. THE SUTTON HOO SHIELD

PLATE IV

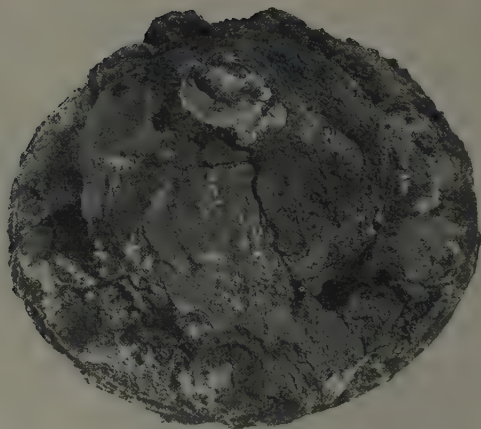


FIG. 6. THE SUTTON HOO SHIELD BOSS, BEFORE CLEANING



FIG. 11. SUTTON HOO: BIRD'S CLAW AND FOUNDATION



FIG. 9. SUTTON HOO SHIELD: THE DRAGON

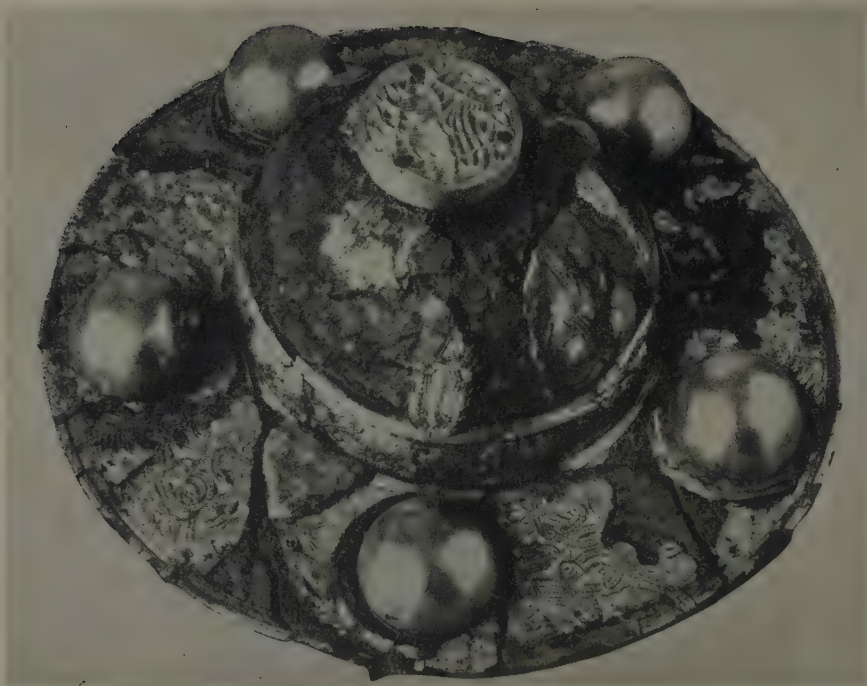


FIG. 12 SHIELD BOSS: VENDEL, GRAVE 12



(a)

(b)

FIG. 8. SUTTON HOO

(a) Detail of Ornament, Sutton Hoo Shield.

(b) Detail of Ornament, Vendel Shield.

PLATE VIII



MARKS LEFT BY CROSS-PLOUGHING IN AN IRON AGE FIELD AT ALRUM, DENMARK (see pages 38-9)
The removal of the mound-layer has revealed the scratches made by the point of the plough on the surface of the underlying white 'biegsand'.

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- (3) the dragon also is unparalleled ;
- (4) in the zoomorphic panels which surround the shield boss the dragon forms are provided with manes (fig. 13), similar to that upon a horse, with separate locks of hair. This motif seems to be absent from later work, in which the mane is indicated by a row of bosses, or a hatched border ;
- (5) in the long gold panel on the dexter side of the shield the grape, as a decorative motif, appears. This motif also seems to be absent from later work ;
- (6) the hemi-spherical gold bosses, with garnet *cloisonné* work, from the sword sheath, have round their base bands of garnet inlays. These garnets have their outer surface serrated. I know of no other example of this technique from the Vendel Age.

It is probable that as work on the Sutton Hoo finds proceeds, quite a number of other aesthetic motifs and technical processes will be found which have no parallels

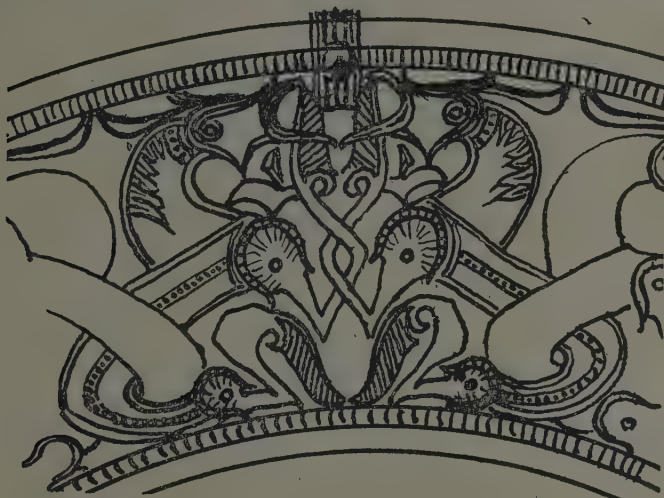


FIG. 13. SUTTON HOO: DETAIL FROM SHIELD BOSS

in the Vendel graves. I have sought for other examples in the art of the migration period which illustrate these motifs, but without success. Others who have greater opportunities and knowledge of the times may be able to solve these problems, and throw light upon the origins of the art of Vendel.

It would seem that the craftsmen responsible for the introduction of these motifs in Vendel art can have left no aesthetic descendants to whom such ideas would appeal. How was that ?

We have seen that in the sixth century the Swedes were engaged in driving their Gothic neighbours from the southern provinces of Sweden and from the Baltic islands. In those disturbed times that direct contact with central and western Europe which the various Gothic tribes had exploited during the previous three centuries would now, for a time, fail to provide a route by which Swedish craftsmen might refresh their minds by contact with artistic developments elsewhere. The original craftsmen died, and their successors developed the Vendel style as we have known it hitherto, without much

fresh material from the south of Europe, and without those interesting motifs which differentiate Sutton Hoo from all the later work at Vendel. The shield and helmet were, of course, parade pieces. In Scandinavian literature we find many appreciative references to 'old weapons', some of them described as being the work of giants. Our examination of the work shows that both the Sutton Hoo shield and the jewellery were old at the time when they were buried. The shield had been extensively repaired. For example, of the twelve dragons' heads which came round the rim, only three were originals, the other nine having been replaced in antiquity by copies in gilded gesso. The gilded bronze dragon in the lower part of the field had the last two inches of his length restored in gesso in ancient times. Some of the dragons' heads on the boss are in gesso—an ancient restoration. Some of the jewellery had been broken and repaired. With these facts in mind we suggest as a probable date for the manufacture of the shield and jewellery the late sixth century; and for the Ship-burial itself 650 or a few years later.

To the last question, 'Who was he', we reply—Can we not see in the man commemorated by the Sutton Hoo burial the younger son of one of the rulers of Vendel? one who, with his father's blessing and rich gifts of old armour and weapons from the hall of his ancestors—gold and silver vessels, fine garments and jewels—accompanied by some 50 or 100 chosen companions, set out in search of a new home in the distant western lands? He and his companions travelled down the waterways of Sweden into the Baltic, through the Sound and round the tip of Denmark, across the North Sea to the fair land of England. Coasting along he found the estuary of the river Deben, in whose sheltered waters, six miles from the open sea, his ship might ride safely. He sent a friendly message to the local ruler, who was glad to welcome such an accession of strength as his visitor could provide. For the times were difficult, and friction between the local tribes severe. So our traveller settled at Sutton Hoo, and the group of eleven barrows testifies to this day that he and his comrades had indeed found that permanent home which they had sought from afar. That his body was not buried among his treasures need cause no surprise. For in the ship burials at Vendel, though the remains of their dogs and pet falcons and other fragile objects have survived, yet in only eight of the fourteen graves was any trace of their owners to be found.

Cerdic and his Ancestors

by P. K. JOHNSTONE

KING Cerdic of Wessex (more accurately, of the Gewissae) is perhaps the most enigmatic figure in the whole of British history. He appears as a chieftain with an Anglian pedigree but an indubitably Welsh name, having Jutish kinsfolk, with whose aid he establishes a Saxon kingdom, which nevertheless seems to remain archaeologically British (i.e. non-Teutonic) for a half-century or more after the supposed date of his conquests. Such a series of anomalies may be treated either as evidence of Cerdic's mythical origin, or as the hallmarks of a reality which is under no obligation to be lucid and uncomplicated. In either case, he symbolizes a chapter of half-forgotten history, obscure but crucial.

Myth of a sort is detectable in Cerdic's section of the Anglo-Saxon Chronicle. It suggests 'learned' speculation rather than the memory of pagan cults. Is Cerdic, like Port and Wightgar an abstraction from place-names? None of the localities which preserved his name were comparable to Portsmouth or Wight as features of the West-Saxon landscape, and in no case is there a reasonable presumption that the name was there before the Saxons came. Always Cerdic's name is joined to an unmistakably Anglo-Saxon vocable—and this in a region where surviving compound names of Celtic origin are not too rare. Moreover, Cerdic is a genuine personal name, not an adaptation. It is, on the whole, easier to accept Cerdic as historical than to explain his invention. Mr O. G. S. Crawford has demonstrated the topographical evidence which goes far toward confirming the record of Cerdic's earlier battles (1), from his landing at Cerdices ora (Anglo-Saxon Chronicle [hereafter Asc] 495) to his victory at Cerdicesford (Charford on the Avon) in the same year that saw him assume kingship. The latter events are dated by the Asc to 519, but the earlier Parker Preface, confirmed by the synchronisms of Ethelweard (2), shows that this date should be reduced to c.501—an adjustment which makes the whole narrative far more acceptable.

After Charford, an eight-year interval ends with a battle of unstated result at Cerdicesleag. If the interval is correctly given (intervals of eight years are suspiciously common) this should be dated c. 509. But where was Cerdic's Lea? The most promising clue yet discovered is found in a document of 1338, which points to the existence of a *Chardele* near Fordingbridge. Since Chardele is an entirely plausible Middle-English version of the name in question, and so near to the site of the previous battle, we seem to find Cerdic still battling to maintain or extend his bridgehead across the Avon, stubbornly opposed by British forces, presumably based on Sarum and Dorchester (3). Cerdic's immediate objective may have been the cutting of the Roman road from Sarum

¹ 'Cerdic and the Cloven Way', *ANTIQUITY* v, 441-58; 'Southampton' *ANTIQUITY*, xvi.

² The Parker Preface is reprinted by Hodgkin, *H. of the Anglo-Saxons*, I, 125f. Ethelweard, Bk. II, cap. IX (188 years from Ine (688) back to Cerdic), III, ii (350 years from Egbert (802) back to Cerdic. St. Augustine's coming (597) is 96 years after Cerdic).

³ *Cal. Pat. Rolls*. Edw. III, p. 150. Roger de Audele, parson at Fordynggebrigge, complains that certain persons (eleven are named) have robbed his house at Wulfedele, and (apparently later) assaulted his servant, Richard *de Chardele*, at Fordingbridge. For this information I am indebted to the Editor.

As a servant (*serviens*) it is most improbable that Richard took his surname from a place very distant from Fordingbridge. If the name survives, it has very probably been distorted by assimilation to the personal name 'Charlie'.

to Badbury Rings. A crossing at Charford enabled him to assail this valuable line of communication at a point roughly midway between the two British strongholds. It is not impossible that he succeeded, since Bokerley Dyke was built by Dark Age Britons to block the road against penetration from the direction of Charford. But Bokerley would also fit the situation prevailing much later.

The following entry (Asc 530=c. 512) is one of the most controversial. It shows Cerdic storming the Roman-built 'Saxon Shore' fort of Wihtgaraburh (Carisbrooke) on the isle of Wight. Subsequent entries (Asc 534, 544) claim that Cerdic turned the island over to his Jutish kinsmen, Stuf and Wihtgar, the latter supposedly being buried at Wihtgaraburh. Actually he must be an eponym of the Jutes of Wight (Wihtgara, Wihtwara). The name of Wihtgaraburh itself is remarkably similar in formation to that of Cantwaraburh (Canterbury). The identity of Cerdic's opponents is not stated (4) but the remark that he slew only a 'few' men is distinctly odd. Tradition seldom if ever minimizes enemy casualties. It might be an indication of strife among the invaders. But let us consider the wider setting.

The campaigns of Cerdic had extended the area of Teutopic control westward along the Channel coast from the Sussex border to the Avon, or beyond. Starting considerably earlier, a similar advance was taking place along the line of the Thames, involving much larger bodies of men. This was the main Saxon drive, of which the Jutes constituted an amphibious southern wing. The whole movement has the look of a planned campaign, The planner presumably would be the Bretwalda, Aelle of Sussex.

But whether Cerdic, Stuf, Bieda and Maegla were field commanders under the orders of a Saxon generalissimo, or independent captains of small pirate squadrons, it is incredible that Wight had been left in British hands until c. 512. The predatory seafarers of the 5th and 6th centuries, like their Viking successors, habitually operated from island bases whenever possible. Though unrecorded (unless Cerdices ora itself was somewhere on Wight) the first Jutish descent on the island must be placed before Cerdic's landing on the Hampshire coast.

At first glance, this would seem to confirm the idea that the fight at Carisbrooke was the climax of a 'civil war' among the Jutes. But it is well to recall that the Britons at this time also possessed leaders of wide views, and that this is the very period in which the existence of a British fleet, based on Dumnonia, is fairly well attested (5).

If Wight served as the Jutish base, that fact would be known to the British command, and what could more effectively cripple their movements than the loss of their base—a loss which would also menace their flank and rear? The West-Saxons proper, the settlers on the Middle Thames, appear to have been pressing westward in force at this time, to judge from the references to fresh arrivals in Armorica between 500 and 514. Southwestern Britain must have been crowded with refugees. If the Dumnonian kings sought to relieve the pressure by colonizing expeditions to Brittany, a more direct stroke at the source of their difficulties is not impossible. It may well have been imperative to forestall an impending co-ordinated attack.

Such a surprise landing on Wight, if initially successful, would force Cerdic to break off his attempts to encircle Sarum or Badbury Rings, and hastily return to defend his base. We may imagine that the British landing force was not reinforced in time. Unable

⁴ Ethelweard, A.530, says they were Britons.

⁵ See J. W. Jeudwine, *The First Twelve Centuries of British Story* (? 1911), pp. 28ff. The references in Welsh tradition to the Dumnonian kings, Geraint and Mark, as 'fleet-commanders', and to Arthur's famous ship, Prydwen, are not without significance. Jordanes (*De Or. Get*, cap. 45) shows a British war-fleet active in West Gaul in 469.

to prevent Cerdic's landing (6), they fell back on the Roman walls of Carisbrooke, just as those who resisted Aelle made use of Anderida. Perhaps 'many' (*fela*) rather than 'few' (*feaga*) men fell at Wihthgaraburh. Even so, the directors of British strategy may have considered the result a costly, but not wholly unsuccessful delaying operation. It was after this that Aelle's plans began to go wrong. If Jutish rule on Wight had been largely overthrown, Cerdic would be delayed by the necessity of re-establishing order, and he would naturally prefer a kinsman (? nephew) like Stuf, of whose reliability he had no doubts. Probably he also felt compelled to leave a stronger garrison behind, reducing his striking force. The effect might be to require a halt until reinforcements could reach Cerdic, thus giving the hard-hit defenders a considerable breathing space.

Carisbrooke is the last victory claimed for Cerdic. He reigned 16 years, according to the Parker Preface, which puts his obit *c.* 517—so close to the probable date of the great British victory at Mons Badonicus as to suggest the possibility of a connexion. In any case, a 'Cerdic's Barrow' (*Ceardicesbeorg*) was known as a landmark in the Andover district, as late as the time of Edward the Elder. Unidentifiable today, it nevertheless suggests that Cerdic died close to the N.W. frontier of his realm—close also to the Roman road connecting Winchester (which, whether still populated or a 'ghost town', must have been in Cerdic's power) and the British stronghold of Cirencester (7).

The Asc represents Cerdic as succeeded by his son Cyn(e)ric, who in fact is associated with Cerdic from 495 on. As regards the earlier entries at least, the association must be unhistorical. It is probable that Cynric's reign really began shortly before 550. In the oldest version of the West-Saxon (or Gewissan) royal pedigree, Cynric is the son of Creoda, son of Cerdic. King Alfred for some reason preferred a recension which omitted Creoda. But a few place-names tend to confirm his existence, suggesting vaguely that his activities radiated northward from the vicinity of Winchester, perhaps as far as the Thames (8). Cynric may have been Creoda's son, or his much younger brother. The latter theory would explain his omission in the genealogy sponsored by Alfred. But his complete suppression by the Asc suggests that for some reason his successors regarded Creoda as a person better forgotten. Could this be for the reason that, after Badon, he had preserved his kingdom by methods resembling those adopted by Alexander Nevsky toward the Mongols? If Creoda, trading on the British element in his ancestry attested by his father's name, made himself *persona grata* to some powerful British ruler, becoming his lieutenant and representative, he may in this way have gained authority over the West-Saxons proper, whose dynasty is likely to have suffered most severely from the result of Badon.

This would help to explain how the Gewissae got control of the much more populous settlements in the Thames valley, and also the southward drift of Saxon settlement in later times—a process which might have begun in the period immediately after Badon, when Creoda's kingdom offered a refuge to the defeated West-Saxons. The much-disputed British rulers of the Chilterns (Asc 571) are much less likely to have been an instance of survival than of reconquest.

⁶ Does the bardic lament for Geraint son of Erbin and his 'brave men from the borders of Devon' who fell at Llongborth—'and ere they were slain, they slew'—refer to this campaign? (*Black Book of Caermarthen*, xxii, *Red Book of Hergest* xiv). One would be glad to have the opinions of Prof. Ifor Williams or Mr Kenneth Jackson on this poem.

⁷ Discussed in my 'Mons Badonicus and Cerdic of Wessex', *ANTIQUITY*, xiii, 92-5, 1939.

⁸ *Crudansceat* near Bentley, and *Creodanhyl* near Alton, Hants. Perhaps also *Crydanbric* (Curbridge) near Witney, Oxon. Kemble, *Cod. Dip.*, 1093, 1035, 1070, 1201.

Creoda is notable for two other reasons. His name, unlike that of Cerdic, appears to be Anglian. It also shows us C-alliteration establishing itself in Cerdic's family. It seems to begin just at this point, for Cerdic's father is named as Elesa.

As to the origin of Cerdic's followers, there can be little doubt. They were, in the main, Jutes from Kent (9)—a fact which suggests that by c. 490 Kent could no longer expand by land, but also that Aelle's Saxon host was busy elsewhere. But was Cerdic himself a Kentishman?

There is a clue to Cerdic's origin which does take us back to the Kent of c. 450. The *tyrannus* Vortigern, in his dealings with Hengist and Horsa, made use of an interpreter, one Ceretic (10) 'and no one save he among the Britons could understand the tongue of the Saxons'. Nothing more is told of the interpreter. While it is by no means impossible that Hengist may have later made use of his services, it is not possible to identify this Ceretic with his Gewissan namesake, without performing a major surgical operation on the chronology of the Asc. Cerdic the son of Elesa was probably born c. 460-470. There remains the possibility of contact between Ceretic and Elesa, who might have married a sister or daughter of the former.

One wonders where Ceretic had picked up his knowledge of the 'Saxon tongue'? Our only clue is his name, which seems to be distinctively Northern. It does not appear in South Britain, so far as I know, until after the Cuneddan migration. About 459 St. Patrick found it necessary to rebuke the British king of Strathclyde, Coroticus, whose sea-raiders (Picts and Scots as well as Britons) had slain some and enslaved others of Patrick's Ulidian converts. This Coroticus figures in the Harleian Genealogies as Ceretic Guletic. He was a contemporary of Cunedda the Votadinian, whose fifth son, Ceredig, gave his name to the county of Cardigan (Welsh Ceredigion, for O. Br. **Coroticianā*). A third bearer of the name belongs to the Cardigan dynasty, while a fourth ruled the kingdom of Elmet in the West Riding, c. 620.

Chronologically, Ceredig son of Cunedda might be Ceretic the interpreter. The possibility that the son of Cunedda was a fluent speaker of some Anglo-Frisian dialect is interesting. It leads one to wonder whether the Pictish component of the mixed Picto-Saxon host routed by St. Germanus in 429 may not have included the Votadini who later reconquered North Wales from the Irish? It may be worth noting that C-alliteration is traceable among all the major North British dynasties (11). It is probably to be considered a British trait (12). Yet Cerdic's father and son bore Anglian names.

Cerdic's original expeditionary force can only have been recruited in Kent, during the reign of Octa, son (?) of Hengist (c. 488-512?). The author of the *Historia Brittonum*, who certainly had access to Northumbrian and Kentish as well as British traditions, says that it was after Hengist's death that Octa came from the North of Britain to make himself king of Kent, to the permanent exclusion of all other descendants of Hengist and Horsa (13). And Geoffrey of Monmouth, in his one unambiguous

⁹ See (Collingwood &) Myres, *Roman Britain and the English Settlements*, pp. 364-6, 397-405. Additional evidence may be found in the place-name Canterton (Cantwara-tum), about 2 miles south of the Cloven Way.

¹⁰ Nennius, *Historia Brittonum*, cap. 37.

¹¹ (a) CYNHIL-CYNLLIB-CEREDIG WLEDIG (Coroticus)-CYNWYD-Dyfnwal-CYNFELYN-CLINOG (b) CUNEDDA-CEREDIG CEREDIGION-CORUN-CARANNOG. (c) COEL-CENEU. See the genealogies in Gould & Fisher, *Lives of the British Saints*, vol. 1, and E. W. B. Nicholson, 'The Dynasty of Cunedag' *Y Cymmrodor*, xxii.

¹² It also occurs among the Mercian rulers (Icel-CNEBBA-CYNEWALD-CREODA) but only after their settlement in Britain.

¹³ H.B., cap. 56.

reference to Cerdic (14) brackets his name with that of Octa. The *gesiths* who enabled Octa to assert his claims so forcefully, coming from the Anglian coastal settlements beyond the Humber, might have included an English Aetheling whose father had married a British lady, and permitted her to name their son for one of her own folk.

There are two other possibilities to be considered. One of these is that (A)Elesa is none other than Aelle of Sussex, two of whose reputed sons (Cissa and Cymen) have C-names. The other is that Elesa was really a Briton (Welsh *Elissyai*, *Elisse*, *Ellis*) perhaps the Elasius (Elafius) who welcomed St. Germanus in 447.

Before making choice among these alternatives, let us consider the names of Cerdic's descendants. If he really was a son of Aelle the Bretwalda, that fact would have been well-known to his 7th century successors. We should expect to find the names Aelle, Cissa, Cymen and Wlencing recurring among West-Saxon kings. Actually, they do not occur, with the dubious exception that a 7th-century *subregulus* of Berkshire was named Cissa. But it is not certain that he was a descendant of Cerdic at all, and in any case Cissa was a notable figure in tradition. We must admit that the Aelle-Elesa equation receives little support, here or elsewhere.

The Elasius theory is a more formidable contender. Yet it hardly fits our requirements. And while Elasius or Elisse *might* be rendered as Elesa by an Anglo-Saxon scribe, the form is not, like Cerdic, definitely British. It recurs in the Bernician genealogy (15) as Aluso, Aluca, the great-great-great-grandfather of king Ida. This *Alusa probably lived about A.D. 400, so that Cerdic's father, with a hypothetical *floruit* of c. 470 could well have been his namesake. This name is not the only link between the dynasties of Wessex and Bernicia. In both, the earliest quasi-historical (i.e., later than Woden) names are Baeldaeg and Brand, so the two lines were ostensibly derived from the same ancestry. Ida is descended from Brand in the tenth generation, Cerdic in the eighth, which would place their common ancestor c. 250. But there are dubious links—eponyms such as Beornec and Gewis, and other names which may be doublets or scribal variants.

Returning to Elesa, whose name suggests a birthplace in or near the continental Angel, where his family might still be in touch with other Bronding dynasties, it seems probable that he was born c. 420–440. This would make him a somewhat younger contemporary of Hengist and Horsa, who were accompanied by 'elders of the Ongul race (16)'. If Elesa belonged to this contingent, he would almost necessarily have become acquainted with Vortigern's interpreter.

A nephew of Hengist, one Ebusa (17), is mentioned as sharing with Octa in the establishment of a 'Saxon' colony somewhere in the North, 'Beyond the *Mare Fre(ne)sicum*' (18) after a voyage in which they sailed round Pictland and 'devastated' the Orkneys (19). If Ebusa could be treated as a scribal error for *Elusa, we would have direct literary testimony as to the activities of Cerdic's father, or a reasonable facsimile thereof, in North Britain. Quite aside from this, the settlement 'next to the

¹⁴ H.R.B., VI, 13, 'Octa, Ebissa and *Cherdich*'. Note the resemblance of this form (which looks very Breton) to the Cheldric(h) who fights at Badon, later on. It is also possible that for Ebissa we should read Elissa, Elesa.

¹⁵ H.B., cap. 57, D. Haigh, *The Conquest of Britain by the Saxons* (1861), p. 132. H.M. Chadwick, *Origin of the English Nation*.

¹⁶ H.B., cap. 37. ¹⁷ H.B., cap. 38. Geoffrey of Monmouth spells it Ebissa.

¹⁸ Discussed by Crawford, *ANTIQUITY* IX, 284, 1935, Collingwood and Myres, op. cit. p. 412.

¹⁹ The Annals of Ulster, A.D. 471 (the date may be too late) note a 'second' Saxon raid on Ireland (the first was in 434) which may be connected with Octa's voyage.

wall which is called Guaul', theoretically as a buffer against Pictish raids, would afford a perfect historical context for the marriage of a North-British noblewoman to an Anglian adventurer.

Esla, Cerdic's grandfather, recalls the Welsh traditions of Osla Gyllellfawr (of the Great Dagger, i.e. *seax*) 'the man who fought against Arthur in the Battle of Badon (20)'. But if, as seems probable, there is any connection, it can only be that the *seax*-wielder of Badon was a namesake of Elesa's father. It is also possible that Esla is nothing but a scribal error for Elesa.

Working backward through the Wessex pedigree, the next name is that of Gewis, eponymus of the Gewissae. As such, he is not necessarily mythical. Ceredig Ceredigion and Lothair of Lorraine are historical, not to mention a large number of Irish instances. If Bede had used the form Gewissings, rather than Gewissae, I would be inclined to accept Gewis. But since his name seems to be taken from that of his tribe, instead of vice versa, his existence becomes highly dubious. It is true that he might, like Ostrogotha the Patient, and Conn of Connacht, have been named for the hereditary subjects of his dynasty. But it is far more likely that the Gewissae (? Confederates) were a formation of the late 5th century, and so had not come into existence at the hypothetical birth-date (c. 370) of Gewis.

This 'weak link' throws some doubt on Cerdic's descent from Freawine and Wig, who figure so vividly in the pages of Saxo Grammaticus, as *duces* of Schleswig under Waermund and Offa of Angel. But the inclusion of Gewis is not necessary to bring the Wessex ancestors back into juxtaposition with their proto-Mercian overlords. Cerdic is fourth in descent from Freawine, while Icel, founder of the Mercian kingship on British soil (21) is fourth from Waermund, Wig's contemporary;

Freawine, fl. c. 360	Waermund, fl. c. 360
Wig, fl. c. 400	Offa, fl. c. 400
Esla	? Angentheow, or Angelgeat
Elesa	Eomaer (? last K. of Angel).
Cerdic, ob. c. 517	Icel, <i>a quo</i> Iclingas
Creoda, ob. c. 547 ?	Cnebba (cp. Danish <i>Knubbe</i>).
Cynric, ob. c. 573 ?	Cynewald
Caewlin, sl. 593	Creoda (sl. 593 ?-Asc)

The two pedigrees thus offer each other some degree of mutual confirmation, since there is no reason to suspect the Anglo-Saxon scribes who preserved them of any attempts at synchronization. Of the earlier names, Brand may perhaps suggest that the very oldest traditions connected the ancestors of Cerdic and Ida with the Danish islet of Brandö.

These indications give us a rather surprisingly clear picture of Cerdic's Continental background. But it is exceedingly improbable that he was born outside Britain. Can we fix his birthplace more definitely than that? Wight was too late a conquest. Kent is

²⁰ Peniarth MS, 118.

²¹ Icklingham, on the river Lark, suggests that Icel's family settled first on the eastern fringe of the Fens.

possible. So are the other early settlements. Place-names offer little help. There are too many of them in Els-, and too widely scattered. Elsham in north Lindsey is perhaps the most attractive, but even if early forms (none of which are at present available to me) should support a derivation from Elesa, there must have been so many others of the same name (e.g. at Elsfield, near Oxford) that its evidential value is very small.

Among the names of Cerdic's descendants, those of Anglo-Saxon origin offer no clues. Those of possible Celtic derivation are surprisingly numerous. Cadda reminds one of the Dumnonian king Cadwy. Caedwalla is too late to help, being probably a reflection of the fame of Cadwallon ab Cadfan of Gwynedd (sl. 634)—an interesting reflection of how little 'racial' prejudice counted, in view of Cadwallon's expressed desire to depopulate Northumbria. The only names of possible value here are those of Cynric and Ceawlin. Cyn(e)ric may not be Celtic at all. Both elements are common enough in Anglo-Saxon nomenclature, though the exact combination is rare. But it does resemble Welsh Cynwrig (*Kenewric* in Giraldus Cambrensis). In any case, a Cynwrig of sufficiently early date to be at all significant has yet to be discovered.

Turning to Ceawlin, we find a name of quite uncertain origin, but one which can hardly be Teutonic of any sort. The combination *-eaw-* recurs in Beaw (for **Beowa*, from *Beowulf*?) and in Gleawanceaster (Gloucester, Glevum). This analogy would suggest a Welsh form something like **Coewlyn* (cp. Caer Gloew, for *Glev-*). The nearest forms which actually occur are *Cocholwyn* and *Cuhelyn*, both supposedly derived from the Irish CuChulainn. But the Northumbrian Bede, though he cites the West Saxon form, preferred to spell the name Caelin (22). He knows it not only as that of the Wessex Bretwalda, but also as the name of a brother of St. Chad (ob. 672). This is particularly significant when it is noted that Caelin's three brothers, Ceadda (St. Chad), St. Cedd and Cynibill all bear names of probably Celtic derivation (23). It is therefore reasonably certain that St. Chad's brother derived his name from local, Celtic tradition, rather than from any echo of the victories of Ceawlin far off to the south.

Caelin may be derived from Celtic **caelos* (Welsh *coel*, 'omen'). Forms *Coelin*, *Coeling* occur in Welsh literature, in the sense of 'descendants of Coel the Old' (24). The name Ceawlin would seem to indicate a claim to Coeling ancestry.

The southward expansion of the Coelings would seem to have been much later than that of the Cuneddians, so it would only be in the land which became Bernicia that an English family could in the 5th century acquire connexions with both Coel and Ceredig. The family history of Cerdic thus goes far to authenticate the often-rejected settlement, near the Wall and the Pictish border. But it would be rash to assume that the settlement owed its origin to the statecraft of Vortigern. For one thing, Octa and 'Ebusa', as the son and nephew respectively, of a man who (according to the Asc—here probably copying an early Kentish regnal table) lived until 488, are probably too young to have founded settlements before friendly relations between Vortigern and his *foederati* were at an end. Cereitic the interpreter is also to be recalled, as are the 'Saxons' who appear in 429 with 'Pictish' allies.

To sum up; Cerdic's ancestors, perhaps once Danish islanders, appear, c. 350-400, as under-kings in Schleswig, marcher-lords to the kings of Angel. Their transference to British soil may be in some way connected with the activities of Hengist and his son (?) Octa. Cerdic was born somewhere on the northeast coast, his mother being of a British family connected with the ruling houses of Coel and Ceredig. He presumably made his mark as a follower of Octa, and joined him in his successful snatch at the Kentish throne.

²² H.E., II, 3.

²³ *ibid.* III, 23.

²⁴ *Book of Taliessin* XLVI. Hengwrt MS, 536.

Notes and News

THE FURROWS IN PREHISTORIC FIELDS IN DENMARK (PLATE VIII)

During the war Prof. Gudmund Hatt of Copenhagen has made some interesting discoveries bearing upon early agriculture in Denmark (1). He had often thought that it should be possible to find traces of the actual furrows made by the plough in prehistoric fields, given suitable conditions for their preservation. In the numerous small fields, however, which he has discovered on the Danish moors, and which correspond to the fields of our so-called Celtic field-system, all traces of actual furrows have long been effaced by soil-changes such as podzolization. But where the original field-surface has subsequently been covered by a layer of material thick enough to prevent such soil-changes there is a possibility that actual plough-marks might be preserved below it. Such a covering layer might consist of drifted sand or of the debris of a later settlement overlying the abandoned cultivation.

Excavating in the spring of 1939 at Alrum in Western Jutland, Prof. Hatt found an occupation-layer 1.5 metres thick which contained superimposed house-sites of the Roman and pre-Roman Iron Ages, resting on the surface of what had previously been a ploughed field. This latter consisted of a layer of sandy mould lying on white 'blegsand' (i.e. pale sand), and when this thin layer of mould was peeled off by the excavators the surface of the underlying 'blegsand' was found to be scored with numerous dark streaks running in two directions more or less at right angles with each other. There can be little doubt that Prof. Hatt is correct in interpreting these streaks as the marks made by the point of the plough, which having penetrated the mould-layer scratched the surface of the underlying white sand, leaving grooves which, becoming filled with dark mould, appear as dark streaks when exposed by the removal of the overlying mould-layer (PLATE VIII, facing p. 29).

One important result of this discovery is the visual confirmation it affords that the small squarish fields of this field-system were in fact ploughed in two directions at right angles to each other, as advised by Pliny (*Nat. Hist.*, 18).

Similar results were obtained at the large Iron Age site at Fjand near Nissum Fjord. Here again were found superimposed house-sites of the Roman and pre-Roman Iron Ages, the lowest of which rested on a layer of drifted sand which in turn overlay what had been a cultivated field, the mould-layer of which contained shards of Iron Age pottery. Upon the removal of this mould-layer the underlying 'blegsand' showed similar dark streaks crossing each other in two directions. In addition to these the drifted sand had covered and preserved two of the low banks or balks that usually delimit the prehistoric fields in Denmark; these banks met one another at right angles, and it was observed that the dark streaks made by the plough in the 'blegsand' were in fact parallel to them, as one would have expected.

Prof. Hatt mentions that in 1937 Dr van Giffen of Groningen found similar plough-marks under an Iron Age settlement-site and grave-mound at Rhee in Holland, published in 1940.

In the light of these discoveries similar dark streaks found by Rosenberg under an

¹ 'Forhistoriske Plovfurer i Jylland', *Aarb. for Nord. Oldkyndighed og Historie*, 1941, 155.

Early Bronze Age barrow at Vesterlund in 1908 assume a somewhat startling significance. Though Rosenberg did not correctly interpret what he found in this case, his meticulously careful work and record has made a re-valuation possible. Here the plough-marks, crossing one another in two directions as before, are seen to work round and avoid a heap of stones which had apparently been collected from the field—a frequent feature in ancient fields in Denmark. Superimposed upon all this was a barrow surrounded by a ring of 14 post-holes some 40 feet across; the primary interment contained a bronze sickle of unspecified type, while a secondary interment in the mound yielded two needles belonging to Sophus Müller's Period III of the Bronze Age. This latter, according to the recent chronology of H. C. Broholm, corresponds approximately with the 14th century B.C., so that we have evidence here of the use of the plough in Denmark, and apparently of the square-plot field system, at a date that cannot be later than this period. This is some centuries earlier than the earliest evidence of its use in Britain, and constitutes a very noteworthy discovery, though it needs confirmation on other sites in order to eliminate the possibility of Rosenberg's data having been misinterpreted.

E. CECIL CURWEN.

IRISH AND HIGHLAND DRESS

It may seem ungrateful for me to make any complaint against a review which is generally so kind, and even flattering, as that in your September number, of my book 'Old Irish and Highland Dress'; but if space permits I should be grateful if you would allow me to correct one or two misconceptions.

In the first place, I never dreamed, as your critic seems to think, of resenting the suggestion that lice existed in 16th century Ireland. That would have been absurd. What I wrote was exactly the reverse. My words (page 54) were: 'No one who served in the trenches in France during the war of 1914-18 will doubt the existence of lice in mediaeval Ireland in abundance . . . The only question is whether the saffron dye was intended as a remedy for this, or was used for the sake of its colour'. I then gave reasons for thinking the latter alternative the more probable of the two.

Nor did I say that Pliny had quoted saffron as a deterrent for lice. I am pretty sure he did not, but I have not got the article by Professor Nuttall from which I drew my information before me, to make sure. What I said was that in a long list compiled by Prof. Nuttall of heterogeneous substances recommended for the purpose 'by various writers from Pliny onwards', saffron only figures three times.

As to my remarks in the preface about 'the vilification and contempt that colours most English writings about Ireland in Tudor times', and repeated in fuller detail on pages 69 and 70 in Chapter v, I am quite sure that if your reviewer had to wade through the pages of Derricke's 'Image of Ireland' and Spenser's 'View of the state of Ireland', or even Fynes Moryson's writings on Ireland (though Moryson was a man of far wider knowledge of the world and more judicial mind than the other two) she would heartily agree. And these three are by far the most important Elizabethan writers on contemporary Irish ways and customs that we possess. Of course there were reasons for this, as well as a good deal of justification, but I was not writing about Irish history or Anglo-Irish relations in the 16th century, but only about Irish dress. My only duty was to state the existence of this attitude of uncritical hostility (I would emphasize 'uncritical') and to gauge its probable effect on their descriptions of Irish dress. And this, to the best of my ability, I did. A more discriminating point of view can be found in writers after the restoration of peace in 1603, such as e.g. Luke Gernon and Sir William Brereton.

H. F. MCCLINTOCK.

ANIMAL BONES FROM ARCHAEOLOGICAL SITES IN BRITAIN

(*Dr J. W. Jackson's gift to the British Museum (Natural History).*)

It has been reported recently that Dr J. Wilfred Jackson has presented his large collection of animal bones from prehistoric and early historic sites in England to the British Museum (Natural History).

This collection is unique. In the course of the last forty years animal bones found in numerous archaeological excavations in Britain have been sent to Dr J. Wilfrid Jackson of Manchester University Museum for investigation and report. In most cases the excavators allowed Dr Jackson to retain the material, and he has thus been able to build up an extensive and unique collection of dated skeletal remains of mammals hunted or husbanded by man down the ages.

Dr Jackson's gift included material from the following archaeologically important sites:—

NEOLITHIC (and in some cases Beaker period). Julliberrie's Grave, Kent; Easton Down flint mine, Wilts.; Harrow Hill flint mine, Sussex; Maiden Castle, Dorset (see below); Skendleby long barrow, Lincs. (ox skeleton); Whitehawk Camp, Sussex.

BRONZE AGE. Amesbury, Wilts (barrow 85 and Ratfyn barrow); Boscombe Down East enclosure, Wilts.; Cambridgeshire Fenland sites; Hayes Wood enclosure, Freshford, Somerset (possibly Iron Age); Minnis Bay foreshore site, Birchington, Kent; 'The Sanctuary', Overton Hill, Wilts.; St. Lawrence College site, Ramsgate; Thorny Down, Wilts.; 'Woodhenge', Wilts.

PRE-ROMAN IRON AGE. All Cannings Cross, Wilts.; Bredon Hill, Glos.; Bury Hill, Hants.; Camerton, Somerset; Cooper's Hole, Cheddar; Colchester (see below); Glastonbury lake-village; Harrow Hill, Sussex; Highdole Hill, Telscombe, Sussex; Kingsdown Camp, Somerset; Maiden Castle (selection only, in part Neolithic); Meon Hill, Stockbridge, Hants.; Swallowcliffe Down, Wilts.; farmstead site, Little Woodbury, Wilts.

ROMANO-BRITISH. Camerton, Somerset (dogs); Colchester (in part Pre-Roman); Dog Holes Cave, Warton Crag, Lancs.; Nuthills 'villa' near Bowood, Wilts.; Park Street 'villa', near St. Albans; Teffont Ewyas Quarry, near Salisbury (dog); Witcombe 'villa', Glos.

MEDIEVAL. Old Sarum (refuse in 11th century cess-pit).

It is understood that Dr Jackson is presenting similar material from Welsh sites to the National Museum of Wales, and from Scottish sites to the Royal Scottish Museum.

The archaeologist often learns much from the osteologist's report even when it is based on a comparatively scrappy series of animal remains from a single site. Thus, from a list of identifications coupled with a numerical assessment showing in what proportions the different types occur, he may learn the relative importance of hunting and husbandry at the particular stage of culture represented by the refuse. If it is revealed that a high proportion of the domesticated animals were killed when young, he will have a further indication of the type of economy of the ancient society under investigation; and so on.

The osteologist, on the other hand, usually requires, if not complete skeletons, at least long series of complete bones of particular animals before he has material on which to base fundamental zoological conclusions. Such series are rarely yielded by single sites, but may be built up by selection of dated material from a large number of sites.

Herein lies the importance of Dr Jackson's collection. Building on the foundations laid by his co-operation with excavators, it should be possible before long for osteologists to trace the development of the breeds of domestic animals in Britain from Neolithic times onwards. At any rate it is clear that the biologist and the archaeologist have here a most fruitful field for further collaboration.

The Council for British Archaeology recently appointed a Committee (the Natural Sciences Committee) to advise on ways and means of furthering the co-operation between archaeologists and those natural scientists whose work has archaeological connexions. In an interim report the Committee pointed out the need for building up central reference collections of, for example, dated animal bones. Dr Jackson's material will form the basis of one such reference collection. It is reported that it will be in the charge of Dr F. C. Fraser.

FARMERS AND FORESTS IN NEOLITHIC EUROPE

Mr R. P. Beckinsale should have verified his facts before claiming to have found a fallacy in footnote 31 of my recent article (March 1945, p. 158). It was precisely in order to avoid inferring anything from 'the existing tree-growth *on* (my *italic*) long barrows' that I omitted from my list such sites as Pole's Wood South (no. 45) and Windmill Tump, Rodmarton (no. 56), where tree-growth was confined to the mound. Instead, I concerned myself with barrows such as West Tump, Brimpsfield (no. 52), of which Witts records bluntly 'This barrow is to be found in the middle of Buckholt Wood (1)'. It naturally happens that in many such the mound itself, *as well as* the surrounding area, is tree-covered, as with Crippets Barrow (no. 23) or Gatcombe Lodge Barrow (no. 27), but this is of course incidental. On the other hand, in at least two out of the nine Cotswold barrows in my list the barrow stands free in the midst of the surrounding wood: thus, of the barrow in Slatepits Copse, Wychwood (no. 68), Crawford wrote explicitly that it 'stands in a clearing in the wood (2)' with only 'small spindle bushes and bracken and a single oak tree growing on it'; again, of the Withington Barrow (no. 57) he stated that it was easily found 'as the whole of that part of the wood in which it lies has now been cut down (3)'.

As to whether, in the case of the long barrows of Cotswold proper, the mound would normally afford conditions more favourable to tree-growth, a question which—*pace* Mr Beckinsale—I was careful not to raise in my foot-note, there is at least room for doubt. Mr Beckinsale's claim that 'by artificially accumulating a *good depth of earth* (my *italic*), the neolithic barrow-builders created conditions most favourable for afforestation' prompts one to wonder whether he has ever seen inside the mounds, many of which from their composition could be described more accurately as stone cairns than as barrows. The entirely separate question, whether the mounds might cause hill-wash to accumulate and so create conditions more favourable for forest-growth on the upper slope, can only be answered individually in the field, although it seems unlikely that barrows occupying the commanding positions specified by Mr Beckinsale would be seriously affected by this factor. In fact, however, all this is rather beside the point. There is an old saying about gilding the lily. It would indeed be hard to improve the tree-bearing capacities of the red clay, which geologists ascribe to natural weathering of the Cotswold limestone and which underlies and surrounds the long

¹ Quoted from Crawford's *The Long Barrows of the Cotswolds*, p. 137.

² *ibid.*, p. 165.

³ *ibid.*, p. 145.

barrows of the region. This clay seam was noted during the excavations at Notgrove (no. 41) and Nympsfield (no. 42), and, as I remarked in my article, it supports a beech-wood with ash and larch not far away from the latter, while samples from the same clay beneath the cairn yielded mollusca indicating 'a rather damp scrub woodland (4)'.

As to the importance of pastoral activities, both in the economy of the neolithic farmers and as an agent in deforestation, I think I made this sufficiently clear in my article (5). The effect of grazing beasts on the form of growing trees has, as a matter of fact, been noted fairly widely. However, livestock exerted their chief influence in hindering or preventing regeneration of woodland.

GRAHAME CLARK.

THE ORIGIN OF SCOTTISH CLAN BADGES

The plant badges which are used by clans of the Scottish Highlands (about sixty are mentioned by different authors) are generally supposed to have been worn by the clansmen in their bonnets to denote their allegiance. This can apply only to the 17th and 18th centuries, for before about 1600 the ordinary Highlander apparently went bare-headed. There was, however, another manner of displaying a clan's emblem. The plant which formed their badge might be set on the top of a staff as an ensign. A bush of heather was thus shown in 1678 by the Macdonalds of Glencoe.

Several considerations, however, suggest that a desire to possess a ready means of identification can hardly have been the sole motive which guided clans in their selection of badges. For example, this purpose seems indifferently served by the adoption of oak by the Camerons as well as by the Stewarts of Appin, their immediate neighbours. Nor does it explain why about half of the clans possessing badges have more than one plant credited to them. Moreover, a good many of the plants mentioned as badges were not very well chosen if they were intended primarily as cognizances, whether worn in the clansmen's bonnets or set on an ensign staff, being either inconspicuous, or apt quickly to wither. We may instance cloudberry, cotton sedge, dryas (mountain avens), rock rose, trailing azalea (*loiseleuria*) and wild thyme. It would certainly seem that some other factor must have originated at least a substantial minority of these emblems.

We begin to appreciate what this factor must have been when we realise that several of the plants which form badges were also in use as charms, chiefly against witchcraft, fairies, cattle disease or plague. Such were ash, bog myrtle, fern, holly, ivy, juniper, oak and rowan. Since none of these is unserviceable as a cognisance, it would appear that plants suitable for this purpose, as well as those unsuitable, might have been adopted as clan emblems primarily because of their magic power. To these charm-badges should probably be added St. John's wort. This plant is called in Gaelic 'the armpit flower of St. Columba' (*Achlasan Chaluim Cille*), having been given, it is said, by the saint to a child herding cattle, to keep under his armpit as a protection against the dangers of the night (M. E. M. Donaldson, *Wanderings in the Western Highlands*, 358). It was originally a pagan charm, on which this legend evidently conferred respectability for Christian use; and it has a special interest for us, because we can be reasonably certain why it was taken as a clan emblem. The clan to whom it is attributed (admittedly by only one authority) are the Mackinnons; and we can hardly doubt that, if this attribution is correct, they adopted it because in the 15th century they were closely associated with the abbacy of Iona, repeatedly furnishing abbots to that monastery. We may therefore

⁴ *Proc. Prehist. Soc.*, 1938, p. 212. Neither barrow should have appeared in my list, having been included in error. Neither, be it added, supports trees!

⁵ *ANTIQUITY*, 1945, pp. 69-70.

conjecture that the other charm-badges also may have come to be associated with particular clans for somewhat similar reasons.

There is, however, a further possibility. The connexion between clan and charm-badge may represent inheritance of the much more ancient and much closer association of tribe and sacred plant or sacred tree, the clan or clans who use the charm-plant as a badge being occupants of that tribe's territory, or in some other way heirs of their traditions (1). For, before they became merely charms, the trees in our list (except apparently holly), and other trees in addition, were certainly held as sacred or semi-sacred by the pagan Celts. By some Celts lesser plants also were venerated: several such plants (including club moss, which is a Highland clan badge) were sacred plants of the Gauls, while other clan badges—birch, bramble, broom and willow—were sacred plants in Scotland (*British Calendar Customs—Scotland*, II, 151). The rowan was a sacred tree of the Irish Druids, as were also the yew and the hawthorn, both these, as well as the rowan, being Scottish clan badges. The nuts of the hazel, another clan emblem, were food for gods and ghosts in the Other World.

In Ireland sacred trees, of which the Gaelic name was *Bile*, were closely associated with the tribes in whose territories they stood, as meeting-places where the chiefs were inaugurated or periodical games were held. Of five such famous trees, mentioned in Irish legend, three were ash, one oak, and one yew. From one of these the tribe called the *Feara Bile* ('the men of the sacred tree') in the province of Meath took their name. In Scotland also the term *Bile* occurs in connexion with ancient burying places (*Cladh Bhile*) and holy wells (*Tobar Bhile*); and it is probable that the old yew at Fortingall in Perthshire is a Scottish example of a sacred tree which was a tribal place of meeting, because on the right bank of the river Lyon, opposite the yew tree and the church of Fortingall, there was formerly a Nemeton, or sacred meeting-place, of which record is preserved in the modern name Duneaves (A. Stewart, *A Highland Parish*, Foreword by Prof. W. J. Watson, VII). The purpose of building a church beside the yew tree was to convert a site formerly sacred to paganism into a Christian sacred site; and it was probably with the same object that St. Kentigern, according to the tradition, hung his bell on another famous tree—that oak which appears in the arms of the City of Glasgow. 'Kentigern's oak', says Dr George Henderson, 'had a sanctity of its own, apart from its use as a Christian belfry' (*Survivals in Belief among the Celts*, 183). Similarly, through its dedication to the saint, the worship, which had previously been rendered to another oak on an island in Loch Maree in Ross-shire as a divinity, was transferred to St. Maol Rubha of Applecross (E. S. Hartland, *The Legend of Perseus*, II, 181-2). Thus some charm-badges at any rate (and other plant badges too, which may never have passed through the stage of being charms) had a previous existence as the 'god-trees' of herb-worship or tree-worship, which worship survived even into modern times in the custom of pilgrims to tie rags to the tree beside a holy well.

If a tribe could take their name from a sacred tree, as the *Feara Bile* did in Ireland, sacred trees might well become tribal emblems. We may guess that this was perhaps

¹ Inheritance by clans of emblems used by the ruling families of provinces of Celtic Scotland seems to have taken place in the following instances, although we cannot identify these emblems as sacred plants:—(1) In Strathearn-Menteith two clans use the same badge, one clan, the Grahams, having held both earldoms; (2) in Argyll the Macdougalls and the Macdonalds, both claiming Somerled, Regulus of Argyll in the 12th century, as their ancestor, have badges which are not very dissimilar; (3) in Lennox perhaps three clans, and in Atholl one clan, are credited with badges that appear to be variants of those used by the Macfarlanes and the Robertsons, who are descendants of the old earls respectively of Lennox and of Atholl.

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the origin of plant emblems as an institution. There was, however, another motive which might associate a plant with a clan or a family. This was the primitive belief that a man's soul could leave his body, and take manifold forms as a separate or 'external' soul, thus avoiding the dangers to which his body was exposed. Instances of this belief are the identification of the hawthorn at Cawdor Castle with the fortunes of the family of Cawdor, of the oak called the Edgewell Tree with the Ramsays of Dalhousie, and of the mistletoe which grew on another great oak at Errol with the fate of the family of the Hays. This last example is specially pertinent, because mistletoe is the clan badge of the Hays; and therefore in this case at any rate a plant badge plainly originated in the conception of an external soul. It is remarkable that even this one definite indication should have survived to confirm other evidence, less certain, that in superstition and magic is to be found an origin of the use of plant badges by the Scottish clans.

W. R. KERMACK.

UNICORNS

I write with reference to Mr Riddell's article '*Concerning Unicorns*', in the December number of *ANTIQUITY* (1945), to point out a possible origin for the legend not mentioned there. A familiar feature on the Mohenjo-daro seals takes the form of an antelope with a single horn curving forward and upwards. This is unlikely to be merely a representation in profile of a two horned animal, as the cattle represented on other seals always have both horns indicated. The practice is not unknown in the Himalayas of doctoring the horns of sheep, so as to cause the two horns to grow together into one, and I suggest that the Mohenjo-daro antelopes were black-buck treated in this way, and formed at least a contribution to the unicorn legend, which probably has a multiple origin in any case.

J. H. HUTTON.

Reviews

PRE-INCAIC HUAMACHUCO. Survey and Excavations in the Region of Huamachuco and Cajabamba. By T. D. McCOWN. University of California Publications in American Archaeology and Ethnology, vol. 39, no. 4. pp. x+223-400, plates 8-23, 21 figures, 1 map. University of California Press, 1945. \$2.00. (London, C.U.P.).

Several of the publications dealing with archaeological projects in Latin America, carried out on behalf of the Institute of Andean Research, have already been noticed in *ANTIQUITY*. The present work is part of Project 9, and it deals with the Andean hinterland of the Moche valley region of Northern Peru. The work consisted of reconnaissance along the Trans-Andine road from Trujillo through Huamachuco to Cajabamba, together with more intensive investigation of selected highland sites, including trial excavations. A description of Uhle's collections of pottery, carved stone, shell and metal, made nearly half a century ago, is included with that of the material obtained by the expedition.

The most important group of sites to be studied was Marca Huamachuco, lying a few kilometres N.W. of the modern town of Huamachuco at a height of over 12,000 feet. Here are evident two periods of pre-Colombian building, and a still older period may be inferred from the sporadic occurrence of isolated ashlar blocks. In addition there are dry stone walls which may be of any date up to the present. Walls of the first or a period are beautifully built of irregular coursed rubble, with clay mortar, and long-and-short quoins after the manner of Saxon work in England. Buildings consist of elongated galleries, grouped in various ways, which had two or more stories, with the floors supported by means of beams or corbels, and in some cases the only entrances seem to have been on the first floor. The most prominent group of buildings, on the Cerro del Castillo, is surrounded by a defensive wall at least, 10 metres high, which, being double, itself constitutes a gallery. The later or B buildings are of inferior masonry and are built among the older ones, in some cases making use of existing walls. They are mostly rectangular, but do not have the characteristic elongated plan of the older galleries. In comparison with coastal sites, finds were not abundant, but it was possible to distinguish between the pottery associated with the A and B types of building. Decorated pottery forms but a small proportion of the sherd material collected, but we are largely dependent on it for correlation with known areas. The A type of building is associated with cursive-painted sherds which have an obvious relation to Kroeber's Cursive Tripod Style of the Middle Period at Moche. In addition, there are negatively-painted sherds whose relationships are not so clear, but which seem to point rather to the type found by Bennett at Gallinazo on the coast than to the better-known Highland type of Recuay. On the strength of this material the period is labelled Middle Huamachuco, leaving a hypothetical Early Huamachuco yet to be found. Other sites, near Lake Sausagocha in the same region, which were not so elaborately fortified, are tentatively ascribed to an earlier phase of the Middle period on rather slender evidence, and the two phases are called Middle Huamachuco I and II. The B buildings are associated with red on white or black-and-red on white sherds which show affinities with Late periods on the coast, and material collected by Uhle points to contacts with Late Chimú. The period is, therefore, called Late Huamachuco.

The other site which was studied in some detail is Viracochapampa, which differs markedly from the other sites in the region in having an extremely regular plan. The masonry has points in common with that at Marca Huamachuco, particularly with the A type though inferior to it; and the plan, whose basic unit is the square court surrounded by narrow galleries, resembles that of Piquillaccta near Cuzco. Pottery is scarce and presents no notable features.

The paper concludes with a brief reconstruction of the life and history of the area in the light of the archaeology and of the records left by the Spanish chroniclers. The people must have lived by agriculture in much the same way as they do now, except that the staple crops are now potatoes, wheat and maize instead of potatoes, maize and oca, while pigs, sheep and cattle now supplement the pre-Colombian llamas, alpacas and guinea pigs. The topography is unsuitable for irrigation, and the Indians have always depended on rains and surface springs for water. These seem inadequate for the large population indicated by Marca Huamachuco, but to this day the Indians make little of very considerable journeys to fetch water and the consumption for washing purposes is notoriously small. Then as now, the Indians must have been hardy people, for though life in the Andes at 12,000 feet can be pleasant enough when the sun shines, the cold and misery produced by the altitude and damp in the frequent periods of mist or rain have to be experienced to be realized.

In Middle Huamachuco times, we can picture a population living in a number of semi-fortified hill villages, with Marca Huamachuco as the capital with a population of, say, two to three thousand living in its galleries. If the fortifications there were to be of any use they must have been built in a comparatively short time, and this implies that there was sufficient organisation to bring in help from the other settlements. Some time about the end of the 12th century this organization broke up, possibly, it is suggested, owing to a succession of dry years, for a slight diminution in rainfall would throw a lot of land out of cultivation. After a short interval, perhaps, a few people returned to the district to live in little scattered houses, including those of Type B huddled among the ruins, in much the same way as they live now. It is probable that this state of affairs persisted until the coming of the Inca some time about the middle of the 15th century. Then, if the chronicler Garcilaso is to be believed, the enlightened chief of Huamachuco welcomed the army of Tupac Yupanqui and, aided by this alien power, forced his people to submit to various reforms, including their gathering into cities. The author considers that Viracochapampa was such a city, and it fits best into the general historical picture as such, but it must be remembered that it is a local product since the sole Inca feature about it is the idea of an ordered plan. The details of the plan are not characteristically Inca, the masonry is not of Inca type and no Inca pottery has been found there. The same applies to Piquillaccta, to which reference has already been made, where both plan and masonry differ strikingly from the Inca types prevailing in the surrounding district, so that one is tempted to explain it as a settlement of Mitimaes from Huamachuco. The methods of the Inca are likened by the author to those of Nazi Germany and the analogy is suggestive, but it must not be pressed too far in view of the vast difference between modern Europeans and pre-Colombian Peruvians, into whose minds we cannot see.

More studies of this kind in the relatively unpromising Andean regions are essential if we are to understand Peruvian archaeology properly, and this work amply repays the rather stiff reading involved in digesting it. There are a few misprints in references to numbers of plates—on p. 256, top line, for pl. 6 read 8; on p. 308, top line of 5th para., for pl. 12 read 18; on p. 314, 3rd line of 2nd para., for pl. 21 read 17; on pl. 17, for o

read s in the second place where it appears. The singular of andenes is not andene but anden, and that of orejones is not orejone but orejon. The plates would be easier to follow if the descriptions were set out clearly in a column, as they perfectly well could be, instead of following on each others heels.

G. H. BUSHNELL.

ANGLO-SAXON ENGLAND. By F. M. STENTON, F.B.A., Professor of Modern History in the University of Reading. Oxford, 1943. 747 pages. 21s.

Every now and then in the cascade that falls from the printing-press there appears a book that may rightly be called a work of real scholarship. Such an one is the long-awaited history of Anglo-Saxon England by Professor Stenton, a masterpiece crowning a life's work of historical research. The outstanding feature which distinguishes this from other histories is the use of the fresh raw material which the labours of a generation of scholars have refined, principally charters and place-names. This is, we think, the first time that a substantial historical text-book has been composed largely of such materials. There are, as the author says in the preface, 'numerous points at which the only clue to the course of events is given by incidental sources of information such as letters and charters'. The art of the historian consists in welding such material into a continuous intelligible narrative, and in selecting from the heap those items which are both authentic and relevant. Professor Stenton has a flair for spotting such items and for extracting from them the last ounce of ore. This flair is closely associated with a vivid and sympathetic historical insight which, to a reader of the book (and perhaps more strongly still in conversation with the author) conveys the impression that he was actually present at the events described. This familiarity is apparent even through the veil of austerity in which the narrative is clothed. The author's style is precise and clear, and his English impeccable; it is, as befits the subject, the style of the judicial summary. He has, too, an eye for geography, and by this we mean, not that he just knows where the places are, but that he is fully aware of the physical environment in which events took place and of their relation to it. That awareness—yet another facet of historical imagination—is unfortunately rare in historians.

Minor points of criticism are perhaps tedious to read, but it is legitimate to express them, though with a feeling that the points raised may have already been considered by the author. He states (p. 23) that 'the Jutish occupation of the mainland has not yet been confirmed by archaeology'. Surely the objects from the Droxford cemetery (*Proc. Soc. Ant. Lond.* 2 S. XIX, 125-9) are to be regarded as of Kentish, i.e. Jutish, affinity? And we think that Jutish types have been claimed at Harnham. During the war an intact pagan cemetery was found (and preserved) in King's Worthy park, a key site; it is to be hoped that its excavation will be carried out and will provide evidence for or against a Jutish settlement in the neighbourhood of Winchester. In citing the evidence of place-names no mention is made of Yting Stoc, the Old English name of Bishop's Stoke; is this valid evidence of the presence there of Jutes?

Again, the place-name 'Englungadene'—not exactly located but between Henley and Watlington—seems to indicate penetration of the Chilterns by Angles from the Wash, confirming Mr Leeds's well-known thesis.

The area over which the citizens of London had hunting rights in Norman times (p. 57) tentatively identified with the kingdom of the Middle Saxons, has been associated by Dr Wheeler with certain linear earthworks that may have defined its boundaries (1).

¹ For the linear earthworks, see *ANTIQUITY* 1931, v, 161-71 and *ibid.* 1934, VIII, 218-22: R. E. M. Wheeler, Guide to Anglo-Saxon remains (London Museum Catalogue, no. 6, 1935).

One of these was certainly in existence in 814. If the inference is correct it is important as indicating that these linear earthworks were used as political boundaries, and may have been constructed for that purpose.

One of the many good points in the book is the author's way of telling us what is not known as well as what is; and of course he is able to do so just because he has himself mastered the whole field of original sources. How often, in the older histories, do we find historical characters mentioned as if they were well known, when in fact nothing whatever may be known about them except their name? Only the learned know how much is unknown, and it is a mark of learning to admit ignorance.

The site of Beandune, where the West Saxon kings Cynegils and Cwichelm fought with the Britons in 614, is still unidentified (p. 63); we wonder whether early forms of the name Bannerdown, on the Foss-way N.E. of Bath, would allow it to be identified with this site?

To the southern examples of 'Grim' names of earthworks may be added a northern one; the Antonine Wall in Scotland was called 'Grimmis dyk' and later corrupted to Graeme's Dyke. And to the Weoh names may be added Wedon or Wadon hill, immediately overlooking the village and prehistoric temple of Avebury, from which the name was doubtless derived. On p. 115, line 2, Yeavening is a misprint for Yeavinger—almost the only misprint we have found in the whole book. (There is a misplaced reference-figure at the bottom of p. 268).

Professor Stenton has mastered the intricacies of social and religious organization, and his account of these, though stiff reading, is most thorough. He describes the origin of the parish church; 'the division of a diocese into parishes, each under the spiritual charge of its own priest, was still a remote ideal in the early eighth century' (p. 147). The nature and origin of minsters is made clear, we think for the first time (pp. 148-9). The whole treatment is objective, but we receive the impression (which medieval studies confirm) that the Church cared more for the things of this world than for anything else. A case in point is the suggestion that soul-scot—that 'portion of the dead man's goods offered for the welfare of his soul by his open grave to the priest of his parish church'—may represent a sublimation, so to speak, of the heathen grave-goods formerly buried and thereby lost to circulation. Tithes were originally voluntary and might be offered to others than priests (pp. 154ff). 'The lands which supported a religious community were exempt from most kinds of secular service' (p. 160), and many pseudo-monasteries came into existence to profit by this exemption. We are reminded of the conversion, in another sense, of country estates into companies to obtain relief from taxation today.

Although so much evidence is derived from incidental sources, Professor Stenton of course uses the great histories to the full extent. Bede's outstanding achievement and greatness is fully recognized in a fine tribute (p. 187). 'In an age when little was attempted beyond the registration of fact, he had reached the conception of history'. Those who, like our author but less deeply, have delved into the writings of the Dark Ages can realize the truth of this. Bede is like an oasis in a wilderness of sand.

King Alfred's greatness is brought out and the interesting point is made that his overlordship represented a union by consent of the parties concerned, previous overlordships being based upon force. It was so of course, but it was forced upon the country by an external danger—the Danes.

The book is worthily produced, and appeared at a time when such production must have imposed a heavy strain on all concerned. It enhances the reputation of the Oxford History of England, of which it is the second volume, and, if that be possible, of the

Oxford University Press. We hope that it may be possible to print more copies soon, as we understand that the whole of the first impression was immediately sold out.

O.G.S.C.

MICHIGAN PAPYRI, VOL. VI. Papyri and Ostraca from Kananis. Edited by HERBERT CHAYYIM YOUTIE and ORSAMUS MERRILL PEARL. pp. xxii+252; 7 plates. Ann Arbor: The University of Michigan Press. London: Humphrey Milford, Oxford University Press, 1944. \$4.00

The contents of the *papyri*, the majority of which are 2nd century A.D., though there are some earlier and some later, are of familiar types, dealing with taxation, sales, actions for trespass, etc. Thus we find that on Feb. 3rd A.D. 154, a lady Valeria Diodora, who is 'about 60' and has a mole near her nose, sells a house and two court-yards to Gaius Julius Niger, an ex-cavalry-man. The main part of the deed is written by her husband, 'because my wife writes slowly'; but she apparently signs the receipt. There is a peculiar interest in a petition (A.D. 197) concerning a summons for trespass. Gemellus complains that one Julius carried away hay from his field. At harvest time Julius, his wife and Zenas again trespassed 'carrying a brephos (=baby) which they subsequently 'threw towards' the complainant. The editors assume, in view of the context, that this was not a live baby, but an embryo, human or otherwise, employed with magical intent either to spoil the harvest or to prevent the owner from gathering it. They cannot produce any exact parallel. Could it possibly have been a 'puppet'?

The *Ostraca*, fragmentary as they are, throw light on many details of daily life. Thus we learn of the existence of a tax on beer, and deliveries of beer, generally 4 *lacci* at a time. The present volume does not tell us the capacity of a *laccos*; in Classical Greek the word sometimes meant a cistern, we find invoices for 'a small jar of wine and 20 loaves of bread'—a proportion which would have disconcerted Falstaff, and for small quantities of oil, meat, barley and other commodities. In connexion with public works there are certificates for so many days' work on embankments; delivery of donkeys, transport of corn to the harbour, etc. One label has simply the words 'by donkeys'—as we might say '*per* passenger train'; finally there is a scrap on which somebody has tried a pen by writing his name twice and misspelling it on both occasions.

The editors are to be congratulated on the care and thoroughness with which they have carried out the work. The translations of the *papyri* are a great help, and the notes give every possible aid to their interpretation. They are well documented. The copious indexes at the end of the volume make reference very easy. J. F. DOBSON.

EARLY INDIAN TERRACOTTAS. By D. H. GORDON. Reprinted from the *Journal of the Indian Society of Oriental Art* (1943), XI, 136-95 and pls. VIII-XVI.

THE DATE OF THE ARYAN ADVENT IN INDIA. By T. G. ARAVAMUTHAN. Reprint from *Quarterly Journal of the Mythic Society*, xxxiv, nos. 2 and 3.

One of the outstanding requirements in Indian archaeology at the present time is the elimination of nonsense. The student of any branch of studies in ancient Indian history and archaeology finds himself confronted with an enormous and verbose literature, which is constantly being added to in a host of journals published in British India and the States, but which on examination proves to be largely erudite word-spinning by writers who show a marked reluctance to deal with objective facts, or to make any direct contact with the soil, the antiquities, or the field monuments of India, but prefer to regurgitate ambiguous passages from the magical texts of their forefathers. Colonel

Gordon's paper is a welcome and valuable attempt to dissipate some of the fog of unfounded speculation which, under the guise of art-criticism, has cloaked the large series of human figurines of baked clay, which should ultimately become valuable dating criteria in early Indian sites; but he is constantly forced to deplore the lack of material derived from dated strata in scientifically conducted excavations, which alone can form a reliable framework for a chronological arrangement. However, he has done what he could with the available data, and has produced a reasonable series as interim classification of a mass of otherwise chaotic material pending the definitive evidence from excavation.

In dealing with the prehistoric figurines he is mainly concerned with those from Mohenjo-daro and Harappa, but illustrates three unpublished and unlocated North Baluchistan figurines in the Peshawar Museum, without however relating them to their analogues associated with black-on-red painted wares in the Zhob Valley; nor does he mention the South Baluchistan examples belonging to the Kulli Buff-ware culture (both series published by Stein). He rightly stresses however the total break in the stylistic series between the Harappa Culture figurines and the so-called 'archaic' series, for which extravagantly early dates have been suggested, but which the trial cuttings in the Sar Dheri 'tell' on the North West Frontier show to be not earlier than the late third century B.C. A note with a section of the Sar Dheri cutting is published as an appendix to Colonel Gordon's paper: a typical 'archaic' figurine was found in an occupation layer above those containing the characteristic pottery (named by Wheeler 'Northern Black Polished Ware') and found on more than one recently excavated site to date from the third and fourth centuries B.C., and was associated with Hellenistic moulded types of clay figures. At Taxila too the type comes from Late Bhir Mound—Early Sirkap levels, having a date in the second century B.C. Colonel Gordon attributes the 'wishful dating' of these figurines to the second millennium B.C. to a desire to provide some tangible Aryan antiquities; but it is worth while mentioning that figurines by no means unlike some of the Indian 'archaic' series do occur at Turang Tepe in northern Iran, and in the third phase of Anau in Russian Turkestan, in a context of about this date and associated with cultures which seem to have some contacts with western India. But the Indian evidence as it stands is clearly against anything other than a very remote derivation, and the chronological gap seems insuperable.

Turning now to the figurines cast from moulds, well known from Muttra and other sites, the author would date these, by comparison of ornaments and other details with sculpture of approximately known date, and by the evidence of two from late in the Bhir Mound sequence, to the mid second and the first century B.C.: to this date too he would assign the Kuvera figures from the North West Frontier Province and thence proceeds to the Hellenistic series, utilizing the material from Seleucia-on-Tigris as a comparative group, the whole ranging from the first century B.C. to the first century A.D. The later figurines are a mixed group which includes the remarkable little 'angel' at Peshawar, whose wings have Gandhara parallels as well as in the paintings at Miran in Chinese Turkestan discovered by Stein.

Mr Aravamuthan's essay is, alas, an all too typical example of the irresponsible theorizing mentioned in our first paragraph, and claims a date for the Indian Aryans in the seventh millennium B.C., with a total disregard of the known sequences of civilization in the other countries of the Ancient East, and relying solely on equivocal hints of untrustworthy legends recorded by Megasthenes and in the turgid depths of the Puranas and the Mahabharata. Such productions would be ludicrous if they were not pathetic: if Indians ever hope to take a creditable part in the study of their own ancient history they

must emerge from their fantasy-world of medieval scholasticism, treat their religio-legendary texts with the same contemptuous scepticism as they would the latest speech by the Viceroy, study India as a part only of the Ancient East, and finally get out into the open air and work on their past at first hand by the methods of modern field archaeology. We did not recover what we know of pre-Roman civilization in Britain by addling our brains over the scraps of information in the classical authors or the Irish legends—we went out and dug it up.

STUART PIGGOTT.

COPTIC EGYPT. Edited by JOHN D. COONEY. *Brooklyn Institute of Arts and Sciences, The Brooklyn Museum, Brooklyn, 1944. pp. 58. \$0.75.*

During the first three months of 1941 there was held at the Brooklyn Museum an exhibition called 'Paganism and Christianity in Egypt: Egyptian Art from the First to the Tenth Centuries A.D.' In connexion with this a group of four papers on various aspects of Coptic Art was read at a symposium. These, which were provided with bibliographies and footnotes, have now been published in the hope that they may serve as an introduction to the study of Coptic Art. Included with them are two additional papers. One is a most interesting study, but all too short, by Dr Dows Dunham on the Romano-Coptic Culture of Meroe. The other is the introduction to the catalogue of the exhibition by Dr John Cooney of the Brooklyn Museum. The Brooklyn Museum is to be congratulated on its enterprise in publishing these papers since they call attention to a much-neglected period in the cultural evolution of Egypt. It is unfortunate, however, that illustrations which were provided by the exhibition itself and in its catalogue, now unluckily out of print, are not available in this useful booklet, for illustrations of selected objects would have doubled its value.

Coptic Art is the name applied to the art of Egypt during the Christian period, that is to say from the official adoption of Christianity by the Roman Empire down to the Arab conquest. A late survival continued in the Coptic Church and its monasteries and among its congregations under Moslem domination, but since it was of necessity the art of an ever-weakening minority its last stages lacked originality and vitality. Naturally the Coptic art of the period stretching from Constantine to Heraclius is too early to be called Byzantine and, though descended from the Greek and Roman art of Ptolemaic and Roman Egypt, is profoundly influenced by the deep-rooted traditions of the people of the Nile.

This innate traditionalism of Egypt and its constant influence on her art and culture is well emphasized by Professor Westermann in his paper on 'The Background of Coptism' and Professor Nock also deals with the same aspect in his 'Later Egyptian Piety'. In Egypt the fundamental basis of art and culture is the same whatever the temporary manifestations may have been. The changes were mostly superficial and what happened on the surface rarely penetrated to the firm substratum of indigenous tradition. Dr Cooney in his contribution provides an admirable introduction to the study of Coptic art, presents to us some of the more serious problems, and wisely warns us how little is really known.

The other two papers on Coptic Painting and Coptic Textiles are more specialised. In the former the student will find a useful and suggestive introduction and the short but well-selected bibliography will guide him where to look for further information. Dr Dimand's classification of Coptic textiles more than any of the other papers suffers from the lack of illustrations, without which it is of course not easy to follow or to check his proposed classification. He gives footnotes but no bibliography, and consequently so standard a work as Wulff and Volbach's splendid catalogue is not quoted. There

is only a passing reference to the well-known Dura textiles (now [1945] excellently published by Miss Bellinger and M. Pfister) which are of great documentary importance for dating the Greco-Roman textiles from Egypt, both pagan and Christian. In Coptic textiles the persistence of the Hellenistic tradition which lingered after the Arab conquest even as late as the Fatimid period is one of the prominent characteristics. Sassanian influence has also to be taken into account. A few examples, for instance the famous fish textile in Lyon, may well be Hellenistic and some of the pieces found in Egypt, for instance those with red warp, may be of Syrian origin. These, however, are problems which need further research.

We welcome the publication of this booklet and hope that it will stimulate increased interest in this obscure period of Egyptian art and will in time call forth a full dress study of Egyptian culture in this age which was an age of transition and of vital importance in the history of Egyptian civilization.

ALAN J. B. WACE.

THE PLEISTOCENE PERIOD: ITS CLIMATE, CHRONOLOGY AND FAUNAL SUCCESSIONS. By F. E. ZEUNER, D.SC., F.Z.S., F.G.S. Monograph of The Ray Society. *Bernard Quaritch*, 1945. £2 2s od.

For some years the author has directed the Department of Geochronology, at the Institute of Archaeology, University of London, and this book is the outcome of his work done there. He has elected to classify the stages of the Ice Age from geological evidence only, without reference to the human industries which are left for another work (*Dating the Past*, *Methuen*, 1945). The human industries, in this country at least, have proved inadequate as dating material, leading to much confusion and the author's plan is justified by its success. The interest to prehistorians will lie in the application to the Ice Age of the absolute time-scale of Milankovitch, which is based on the variation of the heat received from the sun due to the perturbations of the earth's orbit. The curves showing the variation of the heat received in different latitudes during the summer half of the year are found to match closely the climatic oscillations over the last 600,000 years; they have been traced for a million years, bringing us to the beginning of the Pleistocene. Retardation of the advance and retreat of the ice sheets is shown to have occurred, just as our hottest and coldest weather comes after the longest and shortest days in the year. When introduced to English readers some years ago, this chronology was received with some reserve, but it must now be regarded as placed on a firmer basis. A mass of evidence for changes in climate has been collected and examined, mainly from Europe, but the rest of the world has not been neglected. Moraines and other glacial deposits, soils, weathering horizons, cave deposits, aeolian deposits, raised beaches, sunk channels, river terraces and their deposits, and fossil contents have all added their quota to build up a picture of the Ice Age, with its four main glaciations, each divided into two maxima, except the last, which has three maxima, separated by interglacial periods of warm climate and higher sea level, the second interglacial being much longer than the other two. Why the established terms Günz, Mindel, Riss, Würm of Penck and Brückner have been replaced by 'Early, Antepenultimate, Penultimate and Last' with exactly the same meaning is not evident, nor does it seem to have any advantage. One has to think carefully to realize which is the Antepenultimate Interglacial. 'Radiation Minimum' is a concise term to indicate the time when the least amount of the sun's radiation is captured by the earth, but is it sufficiently exact? It implies a change in the sun's radiation, which is not intended. A correlation of the glacial sequence in Britain with that on the continent is attempted, but British workers will look in vain for a solution of the many problems which confront them. The author rightly

stresses the need for further research. An interesting result is deduced by plotting the heights of the raised beaches against the time-scale, namely that the sea level has steadily dropped during the Pleistocene period. Where the water has gone and whether this indicates that the earth is expanding are matters for geophysicists to consider. A chapter is devoted to the fauna of the Pleistocene and the book concludes with a full bibliography, divided into chapters according to the subject matter. A. G. BELL.

CORINTHIAN VASES IN THE HEARST COLLECTION AT SAN SIMEON.

By D. A. AMYX. (University of California. Publications in Classical Archaeology, vol. 1, no. 9). *University of California Press, Berkeley and Los Angeles*, 1943. pp. 207-240, plates 28-32.

This is a detailed and scientific study of four Middle and one Late Corinthian vase in the Hearst Collection. The author, as he says, has not given them mere routine catalogue treatment, but has tried to characterize each piece as accurately as possible and to extract from it fresh knowledge about its context. This has led him to re-examine several of the main groups of the Middle Corinthian style.

The outstanding piece is a splendid pyxis with three handles in the form of female busts which belongs to Payne's 'Delicate Style'. Pyxides of this type are not numerous and the author attempts to arrange the known examples in a chronological series and illustrates for comparison three vases in the Metropolitan Museum, New York.

The second, a flat-bottomed aryballos is an early Middle Corinthian piece. This class of aryballoi has a stylistic kinship with Payne's Chimaera group of plates and the author emphasizing this defines their style as 'Heavy' as opposed to Payne's 'Delicate Style'. The third is a squat oenochoe of which even the stopper has survived. It shows relationship to the works both of the Dodwell Painter and the Scale Pattern Painter, but the author finds its style unique. The fourth, a pyxis, is by a painter christened by the author the Ampersand Painter, of whose work other specimens exist in Amsterdam and Boston. Related to his style are vases in Berkeley, Bucharest, and London. The last piece is a bottle of the Late Corinthian period, a rare shape.

The study is amply provided with references to all pertinent literature and illustrations and the half-tone plates, from photographs by Professor Smyth, are excellent. Naturally it caters for the specialist, but the author has achieved his aim admirably and given us a model of modern scientific detailed cataloguing. ALAN J. B. WACE.

MODERN PROBLEMS IN THE ANCIENT WORLD. By FRANK BURR MARSH, PH.D., late Professor of Ancient History, the University of Texas. *The University of Texas Press: Austin*, 1943. pp. 123. \$1.00

Of the five chapters in this book three, on Roman problems, are based on professional lectures actually delivered; the other two, dealing with Greek life, were added to complete the book, which was prepared in its present form by the author and published after his death. They have the merits and defects of lectures; they are pleasant to read, but in many cases they are too general, and the student who wishes to go further into the subject finds no help in the way of notes or references to authorities.

The first chapter deals with agricultural depression in 7th century Athens. The authorities are vague, and though perhaps more can be got from Solon's poems than from Plutarch or Aristotle, we are in doubt, Prof. Marsh points out, how Solon's reforms actually worked. He gives the impression that more good was actually done by Pisis-tratus, who encouraged the cultivation of the vine and the olive in preference to wheat.

The author next deals with poverty and unemployment in 5th century Athens, and finds that service in the fleet, which gave work to many thousands of men, was an alleviation; but on the other hand the mere possession of the fleet forced the city, after the Persian wars, into imperialistic enterprises for which originally she had no taste.

The Roman section is chiefly concerned with the causes of the downfall of the Republic. Prof. Marsh is dissatisfied with the usual explanations and tries to go deeper. He finds the chief cause in the history of the army. The backbone of the military force was in early days the small-farmer class; this had seriously declined by the middle of the 2nd century, and Tiberius Gracchus tried to revive it. So far is common knowledge, but our author seems to imply that Tiberius' main object was to produce cannon-fodder—and with this we cannot agree. The reforms of Marius were the first step towards the ruin of the Republic, for an army now became the personal retinue of its commander, who held it together by his own strength of character, by the prospect of booty and the promise of allotment of lands to veterans. It became more and more difficult for the Senate to dismiss or replace a General, who might with sure confidence turn his arms against the Government. This theme is developed, and though fair consideration is given to the rise of the equites and the senate's struggle to keep obsolete machinery running, we come back to it at the end, in the refusal of the senate to provide for Pompey's veterans, the unconstitutional first triumvirate and its dissolution leading to the civil war and a military dictatorship.

J. F. DOBSON.

THE HEARST HYDRIA, AN ATTIC FOOTNOTE TO CORINTHIAN HISTORY.

By H. R. W. SMYTH. (University of California Publications in Classical Archaeology, vol. 1, no. 10). *University of California Press, Berkeley and Los Angeles*, 1944. pp. 241-90, plates 33-7.

This study falls into two sections. The first is a detailed description and discussion of the Hearst Hydria, an Attic B.F. vase with Corinthian characteristics. The author treads his way expertly through the minutiae of Attic pottery of the first half of the 6th century to which he makes some important contributions. He touches *inter alia* upon the Lydos-Sakonides question, Tyrrhenian vases, Exekias, the Arkesilas vase, the replacement of the term 'Attico-Corinthian' by Corinthio-Attic, and the Panathenaic amphora in Halle which Beazley excludes from the Lydan group. This last piece he decides cannot be earlier than 566 and from this and other considerations he feels the Hearst Hydria can be dated safely between 560 and 550 B.C.

The second part is an equally detailed discussion of the date of the tyrants of Corinth, one of the most disputed problems of early Greek history. Herodotus says the Kypselids fell in 550, but Apollodorus and other late writers give the date of their fall as 584. When ancient authorities are contradictory, how can we decide? The historical and archaeological factors which can be adduced on either side must also be weighed by an impartial judge. After a careful examination of all the points, textual, historical, archaeological, the author prefers the lower date and the Hearst Hydria, if the author's date for it be accepted, is an excellent peg on which to hang his arguments. On *a priori* grounds it would be safer to trust Herodotus who was nearer in point of time to the Kypselids than to trust late writers like Apollodorus. If it is right to postulate the interdependence of historical events and archaeological facts then the archaeological evidence, even if the economic and historical factors are not always convincing, is certainly in favour of the Herodotean date for the fall of the Kypselids.

The author writes a lively style and introduces apt modern comparisons. His arguments do not suffer from over-ingenuity like some of those of his predecessors. To follow the discussion properly the pamphlet should be read in a well-equipped archaeological library, for everything is thoroughly documented. As an example of modern specialist archaeology used to redress the balance of the literary evidence, this is an illuminating study and no researcher in 6th century Greece can afford to neglect it.

ALAN J. B. WACE.

CEREALS IN GREAT BRITAIN AND IRELAND IN PREHISTORIC AND EARLY HISTORIC TIMES. By KNUD JESSEN and HANS HELBÆK. *Det Kongelige Danske Videnskabernes Selskab, Biologiske Skrifter, Bd. III, Nr. 2. Copenhagen: Ejnar Munksgaard, 1944. pp. 68 and 26 figs. Kr. 8.50.*

This monograph, the work of a botanist and an archaeologist working in co-operation, will be studied by their British colleagues with the greatest interest. The comparatively uncommon finds of ancient carbonized grain that have been made in Britain have already been examined and reported upon by British botanists, but this work represents an attempt to supplement these discoveries by searching for and identifying the impressions of grain, etc., that occur on the surfaces of hand-made pottery vessels more frequently than we sometimes realize (see *ANTIQUITY*, 1941, xv, 199). In addition some of the carbonized grain has been re-examined by the Danish workers, and their findings do not tally with those already published by our own botanists (1). But in the process of carbonization the actual and relative dimensions of the grain suffer some distortion, so that the authors consider that plastilina casts of impressions of grains preserved in baked clay, as in the case of hand-made pottery, provide a more reliable basis for diagnosis. Besides this the pottery itself is often datable, thus automatically dating the grain-impressions.

For this purpose Hans Helbæk visited fourteen of the principal museums in the British Isles in 1939, studying all the sherds of hand-made pottery which could be dated by form or associations, in search of grain-impressions. Unfortunately, for one reason or another, he was not able to visit five of our most important provincial collections of early pottery, viz. the museums at Avebury, Devizes, Salisbury, Dorchester and Northampton. Even so he discovered 210 pots or sherds bearing 426 impressions, mostly of cereals, but also of some utility plants (4 impressions). This number is not large enough to serve as a basis for sweeping conclusions, as the authors recognize, even if it be supplemented by the 3,442 carbonized grains which they examined.

The results are set forth in a series of tables comprising impressions of cereals, carbonized cereals, and remains of weeds, respectively, each table being subdivided according to period (2) and find-spot. Further tables summarize the results as totals and percentages, and from these the following data are perhaps the most significant. The relative frequency of the various cereals for all periods together, distinguishing impressions from carbonized grain, is as follows: Barley, 78 per cent (impressions) and 36 per cent (carbonized); Emmer, 8 and 4 per cent respectively; Bread Wheat, 0.9 per cent (impressions); not separately listed among the carbonized grain, where 'various wheats'

¹ In particular the authors disagree with the oft-recurring diagnosis of 'bread-wheat' made by British botanists, claiming that the specimens which they have re-examined contain a mixture of various grains.

² Unfortunately, presumably due to some misunderstanding, the Deverel-Rimbury culture is grouped with the Early Iron Age instead of with Late Bronze Age.

amount to 46 per cent) ; Spelt, 0.2 and 1.0 per cent ; Oats, 6 and 9 per cent. As regards the relative frequency of the various cereals in each period it is to be noted that, while in the Neolithic period Emmer and Naked Barley appears to have predominated, Naked Barley is markedly predominant throughout all phases of the Bronze Age, with Husked Barley as a good second ; Emmer comes to the fore again in the Early Iron Age, while Husked Barley takes the lead, probably in the Roman period, and certainly in the Saxon period. While ' various wheats ' are second in importance in the Roman period, Bread Wheat forms only a small percentage of the total in the other periods. Spelt and Oats first appear in the Early Iron Age, and Rye is first listed in the Roman period ; Oats are second in importance to Husked Barley in the Saxon period. The corresponding totals for Denmark are also given for comparison, and here we find Emmer leading in the Neolithic period, Naked Barley in the Bronze and pre-Roman Iron Ages, and Husked Barley in the Roman Iron Age, with Oats and Millet first appearing in the Late Bronze Age, and Rye in the Roman Iron Age. These correspondences are very close ; perhaps the most noteworthy difference is the absence of an Emmer peak in the Danish pre-Roman Iron Age (corresponding to our Early Iron Age).

The above data apply to the British Isles as a whole and cannot be taken as necessarily representative of southern England, because (1) five of the most important collections of English prehistoric material were not examined (as already noted), and (2) a large proportion of the material examined comes from Scotland, the climate of which does not favour the growth of wheat.

It is also to be noted that impressions of Flax occur during the Bronze Age—one in the Middle and one in the Late Bronze Age.

The last section of the work is devoted to a detailed discussion of the plant remains and the criteria by which they may be recognized, illustrated by enlarged drawings of grains and spikelets, etc., and furnished with tables of maximum, minimum and average dimensions and distribution maps for the occurrence of each cereal in the British Isles.

This study, though regrettably incomplete, is marked by that meticulous care and attention to detail for which our Danish friends are noted. The most alarming feature is the wide discrepancy between their conclusions and those of British botanists regarding the same samples of carbonized grain. We look forward to hearing the views of British botanists on this matter.

E. CECIL CURWEN.